12

HARWELL
UK ATOMIC ENERGY AUTHORITY

*** STRICTLY EMARGOED UNTIL 17.00 THURSDAY 15 JUNE 1989 ***

HARWELL ENDS COLD FUSION RESEARCH PROGRAMME

Following a comprehensive programme of electrochemical experiments, Harwell Laboratory has decided to end research into cold fusion by this technique.

Dr. Ron Bullough, FRS, Chief Scientist of the AEA who initiated Harwell's work in March of this year said, "The potential benefit and scientific interest in cold fusion, together with Government's need for information and advice meant that the subject had to be investigated. However, results to-date have been disappointing and we can no longer justify devoting further resources in this area."

In the absence of new information, Harwell do not intend to re-open work on cold fusion.

Dr. Bullough went on to say, "This work demonstrated our capacity to mount a thorough programme in basic science at short notice and the ability to put together a sophisticated cocktail of scientific expertise and equipment is the unique attribute of the AEA."

A brief paper will be published in an appropriate scientific journal and a full assessment and report of Harwell's work will be available through HMSO in due course.

89/26

15 June 1989

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Summary of 'Cold Fusion' Work at Harwell Laboratory

1. Neutron measurements

Our measurements are designed to achieve high sensitivity coupled with measurement of background at the same time and place as the sample measurement, together with careful checks for artifacts.

First work employed the 4π neutron counter (efficiency 50%) with display of output on a chart recorder. Detection sensitivity was around 1 neutron per second from the specimens.

a) In preliminary work, first a single cell comprising a bent, 1mm diameter Pd wire cathode and Pt anode was electrolysed inside the 4π neutron counter for 10 days.

A set of 3 cells comprising the one above, with an attempt at surface poisoning, a cell with a 1mm thick plate and Pt anode, and a cell with a new 1mm diameter wire and Pt(Rh) anode were electrolysed inside the 4π counter for 10 days.

b) An automatically controlled trolley for sample - blank interchange has been constructed for the 4π neutron counter. A powered and an identical unpowered electrochemical cell are exchanged in the counter every 5min. This period can be varied at will. Data is collected by computer and also displayed on chart recorders. This arrangement allows detection sensitivity ~ 0.05 neutron per second from the specimens

Specimens examined :-

Pd foil in $LiOD/D_2O$
- with surface recombination poisons
- with pre-electrolysed solutions
- with Pt and Au anodes
- with vacuum annealed metal

Ti rod
- with Au anode
- with D_2SO_4, $D_2SO_4/PdCl_2$ and "Jones Brew"

Ti granules
- with "Jones Brew" and Au anode
- with D_2SO_4 and Pt anode
- with D_2SO_4 and deuterium uptake promoter (sodium polyphosphate) and Pt anode

TiFe (a known hydrogen storage material)

UPt_3 and $CeAl_2$ ("heavy electron metals")

Pd specimens of different metallurgical structure (checks assertions that this is important - 8 different types)

Pd specimens with D periodically electrochemically loaded and then stripped (checks assertions that dynamic situations and moving phase boundaries are important)

Pd specimens that have been plastically deformed (checks assertions that fresh dislocations are important)

c) A simple counting assembly comprising 8 cells in 2 water baths, fitted with ^3He neutron detectors, and γ counters thresholded to display all counts over 1.75MeV was constructed. This assembly was capable of detecting emissions at the level of a few hundred events per second.

Specimens examined :-

1. 2mm x 10cm Pd rod; 64mA cm^{-2}; Pt anode
2. 1mm x 10cm Pd rod; 36hr at 64mA cm^{-2} followed by cycle 64 $\leftrightarrow$ 6.4 mA hourly; Pt anode

b) 'Isothermal'

This work uses a calorimeter designed for nuclear safeguards work. It has a large specimen chamber into which a cell with 2 litre electrolyte capacity has been fitted. Anode and cathode gases are kept separate and are lead out of the calorimeter separately. The large electrolyte volume means that topping up is unnecessary. The control system measures the power required to maintain the chamber at constant temperature (42C) : power emission from the cell causes a reduction in this. This means of operation eliminates the sloping baseline which is a characteristic of the simple Dewar calorimeter. Heat flow out of the sample space is strictly radial. The calorimeter has high stability and resolution - σ is ±8mW in 20W.

specimens examined (0.1MLiOD) :-

2mm Pd rod
arc remelted Pd beads
2mm rod annealed in vacuum and preloaded with D_2 from the gas phase
fourth type of cast Pd
Pt control specimen
variety of current densities

3. Electrochemical support

Characterisation of penetration times for D into Pd and Ti; dependence of loading on solution composition and effect of surface hydrogen recombination poisons; measurement of potential-time transient after current interruption to characterise D loading.

4. Analytical support

a) measurement of D in Pd by hot extraction mass spectrometry (specimen frozen in liquid nitrogen upon removal from the electrochemical cell in order to retain D).

b) measurement of ^4He in Pd by mass spectrometry; calibration by "standard additions" of He to cathodes using ion implantation.

c) isotopic purity, Li content (including isotopic content), and trace impurity content of electrolytes.

d) measurement of tritium in electrolytes.

e) optical and scanning electron microscopy of electrodes; electron probe microanalysis.

f) SIMS (for Li profile and distribution) and ESCA of electrode surfaces; SIMS calibration against an ion implanted standard.

g) check for neutron activation products; calibration by exposure of Pd to a standard neutron source (10^4 s^{-1}), in a tube, immersed in a water bath.

5. Attempted repeats of the 'Frascati' experiment

The detector in 1d above was used in this work, which was a collaboration with Culham Laboratory and JET. Counting was also carried out at JET. Ten different experiments were performed, using a variety of conditions of gas loading and temperature-time history.

3. 1mm x 10cm Pd spiral, surface poisoned, 64mA cm^{-2}; Pt anode
4. 1mm x 15cm Pd wire ; as 2 but cycled every 5 min; Pt(Rh) anode
5. 1mm x 5cm Pd wire; 500mA cm^{-2}; Pt(Rh) anode
6. 4cm x 1cm x 0.1mm foil; graphite anode
7. 0.15cm x 8 cm Pd(Ag) tube; Pt(Rh) anode 100 mA cm^{-2}
8. 0.15cm x 8 cm Pd(Ag) tube; Pt(Rh) anode; various cycles including alternate anodic/ cathodic cycles.

All cells had 0.1M LiOD /D_2O electrolyte
The anode in cell 6 disintegrated after some days and had contaminated the cathode with copper. The cell was replaced with the cell containing the plate from a) above, after the plate had been abraded, with electrolyte concentration about 1M LiOD and run at 160 mA cm^{-2} (2A total current). After several weeks the electrolyte concentration in this cell was reduced to 0.2M and the current density to 16mA cm^{-2}.

d) A double chamber neutron counter has been constructed. This features two cavities of dimension 40cm x 25cm x 30cm deep, side by side , constructed of borated resin blocks 15cm thick, and lined with Cd sheet and polythene. Each cavity has 16 high-pressure ^3He counters arranged in two banks of 8, sealed in triple polythene bags and dessicated. One cavity counts the specimen whilst the other counts the background. Data is displayed on chart recorders and also collected by computer. Counting sensitivity is 4%, corresponding to a few neutron per second inside the cavity. The cells from b) above were transferred to this detector after about 6 weeks of electrolysis

e) A sensitive γ counting assembly has been constructed, with a large intrinsic Ge detector surrounded by a thick lead shield. This detector has been used to search for 5MeV γ from the p-d reaction, using Pd and Ti cathodes in mixed D_2O/ H_2O electrolyte. (The p - d reaction has higher cross-section than the d - d reaction, at low energy).

2. Calorimetry

a) 'Pons-Fleischmann' heat-flow calorimeters

16 calorimeters comprising glass Dewar flasks held in a high-stability water bath (design similar to those used by Pons and Fleischmann) have been constructed. Full data-logging is used.

Samples examined :

1, 2, 4, 6mm diameter rods ('specpure') from Johnson Matthey

0.1M LiOD / D_2O
0.1M LiOH / H_2O
0.1M NaOD / D_2O
0.1M NaOH / H_2O

Neutron counters are placed above these assemblies

b) Improved heat-flow calorimeters

These have larger internal volume, with the cells surrounded by an aluminium case whose temperature is measured. The case integrates the heat flow from the cell and measurement of the temperature of the case eliminates the sloping background and temperature gradients which bedevil measurements with the Pons-Fleischmann design. They also have larger separation between anode and cathode and are insulated to ensure radial heat flow only from the sample space.

Specimens examined (0.1M LiOD) :-

2mm 'specpure' Pd rod
3 different types of cast Pd

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Telephone: 617-495-2864
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Fax: 617-495-0416

June 14, 1989

Mr. William Woodard, Secretary
Cold Fusion Panel
ER-6, 3F-043
Department of Energy
FAX 202-586-3119

Dear Bill:

I am sending with this message a copy of reports frome those who went to Utah. Please FAX all 12 pages to all members of the Cold Fusion Panel as soon as possible. I have only limited FAX facilities here.

I am also sending you acopy of a letter that John and I have each received from Bockris. Due to my being in Chicago I did not receive it until two days ago and since then I have been trying unsuccessfully to reach John Huizenga to discuss it. I have no objection to having some of Bockris' candidates added, though the committee is large at present. However, it would be good to have a membership which he considered fair. Incidentally I do not know how he gets a "united opinion" at Harvard. I've almost never found it possible at Harvard to obtain a united opinion on anythig and as far as I know there have been relatively few Harvard statements on this subject.

Sincerely,

Norman

Norman Ramsey

To Members of the Cold Fusion Panel

5/1003
F

Lyman Physics Laboratory
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June 14, 1989

TO: Members of the ERAB Cold Fusion Panel
FROM: Norman Ramsey
SUBJECT: Visit to the University of Utah

On June 2, 1989 the following members of our panel visited the University of Utah: Allen Bard, William Happer, Jr., Barry Miller, Larry Faulkner and Norman Ramsey along with Tom Finn and David Goodwin. After an initial discussion with Vice-President Brophy we visited the laboratories of Pons before lunch and had lunch while discussing with Dean Wadsworth his experiments.

Since there has been no opportunity for the visitors to discuss a combined report subsequent to the preparation of individual reports and since there is only a very short time before the next meeting of the Panel, it seems best to distribute subgroup and individual reports as the report of the meeting. Therefore, accompanying this report from me there are a joint report by Allen Bard, Larry Faulkner and Barry Miller and an individual report by Will Happer. Reports by Tom Finn and David Goodwin have already been distributed.

My comments in this report will be limited to a stastement of my own views with no effort to synthesize it with the reports I have received from others. To avoid undue repetition, I shall omit factual information contained in the reports of others.

On the average I was more favorably impressed by the work in Pons laboratory than I had expected from the information I had received in advance. The laboratory is reasonably well equipped with instrumentaion and Pons and his associates have had considerable experience in electrochemistry. They have now had a number of high heat runs with the output claimed as positive well above the disputed 1.54 volts correction. Pons stated that whereas he was 70% sure he had excess heat when they made their first announcement he was now 99% sure. He claimed that the heat out in some runs now exceeded the toal energy that had been put in, but it

was not completely clear that this included all the energy in the palladium preparatory stage.

No cell was operating at high heat output during our visit, presumably due to a power failure two nights previous to our visit.

I believe that the Committee did not locate an error in their work that was so conspicuous that all present could agree that it invalidated all previous results. However, heat measurements are notably treacherous and it seemed to me that there were possibilities to obtain false results.

In my opinion the weakest portion of the experiment arose from the fact that the heat generated was measured by determing the temperature of the cell and calculating the heat generated from this and from the coefficient of heat transfer to the surroundings. An error in this coefficient or an undetected change in its value would lead to a corresponding change in the calculated heat output. The heat transfer path was made complicated by the fact that the outer shells of some of his cells were evacuated which meant that the heat transfer was in part by a round about path through the top of the cell. Subsequent to our visit I wrote Pons a letter asking about the frequency of his measurements of the heat transfer coefficient and whether they were regularly made during periods of high heat generation. So far there has been no time for a response.

Another weakness is that the cell is not operated as a closed system with the deuterium and oxygen being catalytically combined and with overall heat generation from the closed system measured over a long period of time. Pons agreed that a closed system would be better and he hopes to have one in the future.

My net appraisal of the heat measurements in Pons laboratory is that on the basis of this one visit I can not prove they are in error, but on the other hand I am not convinced they are valid.

We had lunch while discussing with Dean Wadsworth his experiments and those of his collaborators. These experiments were primarily intended to produce activated electrodes on which they could do metallurgical studies. As a result they were not designed to give convincing proof of the existence of high energy output. Originally their experiments showed no evidence for high energy output but subsequently they have had two major excursions with

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high heat output. Will Happer pointed out an anomaly of their measurements in that during one of the high heat excursions while the cell was being operated at constant current, the voltage did not measurably change even though it should due to the dependence of the resistance on temperature. Later I received a phone call from a member of his staff who said they had found that their constant current source had the characteristic that at the level observed it changed from being constant current to constant votage. They were investigating the implications of this observation on their measurements.

While we were in Dean Wadsworth's laboratory, Dr. Michael Salaman of the Physics Department, who has only recently been allowed to place counters adjacent to Pons' cells and has not yet had the opportunity to analyze the data and the controls of his experiment during the time in which a cell was boiling. He has a sodium iodide scintillator beneath the water bath with the cells and a plastic sandwich of uranium-235 to detect the neutron induced fission by tracks in the etched plastic. These observations should give a good determination of neutrons and gamma rays, including any that would be produced by the fusion of two deuterons directly to helium-4 provided the excess energy comes off in the form of gamma rays. Although Salaman's data has not yet been extensively analyzed, he feels that it is well below the level one would expect if Pons' heat results were due to any normal fission process. I believe we should be careful about quoting Salaman publicly by name or by implication. He appears to be an excellent source of valuable information and it would be a great loss if his experiments were terminated by strained relations between himself and Pons.

Salaman's data can not eliminate the possibility of the production of helium-4 with all the heat going directly to the lattice as in the theories of Simon and Walling which are endorsed by the President of the University. There was thought to be some early experimental evidence for the production of large amounts of helium-4, but I believe this report has now been withdrawn due to contamination of the mass spectrometer with helium. If Salamon's preliminary results hold and if the Pons' claims remain, clearly one of the greatest needs is for a reliable determination of the helium-4 produced. Unfortunately, this seems to be somewhat complicated by agreements with the palladium supplier that he should do the analyses of the used electrodes.

My net conclusion from the total vist is that we have not in that one visit disproved Pons' claims. On the other hand, there are real potential sources of error and I am not convinced the experiments are valid. The initial nuclear data makes it particularly difficult to understand how there can be fusion, though data on helium-4 is needed to make this argument water tight. In my opinion what is necessary to get the question resolved is for the Pons group to work with a well equipped and experienced energy lab such as Los Alamos or Argonne in a jiont experiment where the differences can be resolved with a palladium electrode that is working by the measurements of the Pons group.

Norman

Norman F. Ramsey

9/1003
K

CONFIDENTIAL TO THE DOE PANEL ON COLD FUSION

Report on Salt Lake City Visit
A.J. Bard. L.R. Faulkner, and B. Miller

Date of Visit: June 2, 1989

Date of Report: June 8, 1989

The visit with Pons clarified a number of important points about his experiments and claims. We were able to observe many details of his experimental arrangements and measurements practices, and it was possible to question him about experimental calibrations (a particularly crucial matter) and about his use of materials.

Pons's claim that some cells produce excess heat rests upon a comparison between the measured heat power evolved from the cell and the electrical power input. The latter quantity is precisely determinable as the product of the current through the cell and the voltage across it, both readily measured at four significant figures. Pons's comparisons normally involve a correction of this quantity by subtraction of 1.54 V times the cell current, to account for the enthalpy leaving the cell as vented D_2 and O_2. The underlying assumption is that 100% of the current goes to produce D_2 and O_2, which is vented perfectly.

The conclusions turn to a large degree on the quantitative accuracy of the measured heat evolved. A principal value of the visit was in learning exactly how this is done. It has been evident from public reports that the primary data are temperature differentials between the interior of the cell and the external isothermal bath. What has not been clear is the calibration linking these figures to the heat power evolved in the cell.

The calibration method is based on the temporary addition of a power increment via a resistive element in the cell. This ΔP causes a change in the steady-state temperature by an amount ΔT, then a differential heat transfer coefficient $k_D = \Delta P/\Delta T$ is calculated. Pons can carry out this measurement as the cell is running, but he seems to do so only irregularly. The resistive power increment is added on top of that from the electrolytic process, thus maintaining similar stirring conditions for power dissipation. Once k_D is determined, the heat power evolved from the electrochemical action is calculated as the product of k_D and the difference in temperature between the operating cell (with the calibration heater off) and the external bath. This temperature differential was shown to reach 30° or more, but this can be true both from favorable cases (excess heat) or high input current densities. The cells have a vacuum jacket to reduce heat transfer to the thermostatted bath. Particularly at higher temperature, the major heat loss is through the head space leads and evolved gases. Pons said higher temperature experiments were performed with air jackets rather than vacuum, but for the "better case" cell

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considered below, a vacuum jacket was used. The time constant of these jacketed cells appears to be about one-half hour. Calibration cycle times were mentioned as twelve hours.

The calibration involves a temperature rise of about 2°, measurable to 0.01°, hence the calculated heat power values have at most three significant figures.

In principle and in practice, the differential coefficient k_D depends on the magnitude of the difference in temperature between bath and cell. In using the value of k_D at the operating point to calculate the evolved heat power, Pons is assuming that k_D remains constant over the range of cell temperatures from the value of the bath to the operating point. His real need is for the integral heat transfer coefficient k_I at the operating temperature T_c, which is truly related to the evolved heat power P as $k_I(T_c-T_b)$, where T_b is the bath temperature. The differential value k_D is related to k_I as:

$$k_D = \int_{T_b}^{T_c} k_D dT / \int_{T_b}^{T_c} dT$$

Although Pons and Fleischmann present a mathematical model indicating their understanding of the thermal engineering of the cell, their experimental approach appears to lump all effects into the measured k_D.

The use of k_D in place of k_I produces a significant uncertainty in Pons's estimation of evolved heat. The uncertainty is large enough to cast doubt on the existence of excess heat in all but the most substantial cases reported by him. For example, we were taken through a "better case" with the following operating characteristics:

1.		
2.	Electrode:	0.4 cm dia. x 1.25 cm rod
3.	Current:	600 mA (380 mA/cm^2)
4.	Voltage:	9.380 V
5.	Cell Temperature:	66.30°
6.	Bath Temperature:	30.00°
7.	k_D:	0.184 W/°C
8.	Evolved heat power:	6.682 W
9.	Electrical input power:	5.628 W
10.	Excess heat:	1.054 W (net above electrical input)
11.	Power produced as D_2 and O_2:	0.924 W
	Pons's excess heat:	1.978 W

In this case, the excess heat was 19% above input power, even uncorrected for evolved products (Item 9 as a percentage of Item 8), and 35% above input power assuming 100% evolved products (Item 11 as a percentage of Item 8). If k_I were 19-35% lower than k_D at the operating point (i.e. k_D = 0.153 - 0.130 w/°C), the excess heat would be nil. In fact k_D is a sufficiently strong function of temperature to believe that this might be true. Pons was not able

to show a calibration curve for k_D vs. T for the cell of interest over the temperature range from 30-70°. In general, k_D increases with $T_c - T_b$, so Pons's practice of assuming $k_D = k_I$ is expected to overestimate the evolved heat power.

Pons's methodology is, in principle, capable of defining the evolved heat accurately. He needs to carry out calibration frequently enough over the operating range to be able to calculate k_I directly. We were shown that they have examined the differential value over many steps for some cells, but it is not clear that this refinement has been applied to the k_D-k_I issue in published data.

On the other hand, the defect in current practice is not a satisfactory basis for dismissing the extraordinary evolved heat power that he reports in bursts lasting days or more. He provided data in which the heat power exceeded input power by more than an order of magnitude. This is too large a discrepancy to assign to misassignment of k_I as k_D. The cell voltage during the burst record shown to us decreased, consistent with an electrolyte resistance drop and lowered overpotentials for such temperature increases.

Under questioning, Pons provided a good deal of other useful information. The main points are as follows:

(1) He is committed to allowing several laboratories the opportunity to analyse pieces of Pd for ^4He. Results should be available within a month.

(2) A cell demonstrated to show excess heat is now in the hands of investigators outside Pons's laboratories. He declined to identify the group holding it.

(3) Pons declares that he finds no excess heat in a cell containing H_2O in place of D_2O. He says that previous confusion over this control arose because he chose to use, in earlier experiments, an electrode that had been active in a D_2O cell. Pons could not show us the detailed data on the H_2O cell(s). These data would be useful, because they would serve as a measure of the accuracy of the calorimetric procedure.

(4) There were a number of comments about materials:

(a) His Li has been drawn from a single old (1978) sample, which was stored dry. Attention was paid to maintaining an argon atmosphere while dissolving the Lithium to make the stock solution so that CO_2 absorption should have been minimized.

(b) His D_2O has come uniformly from Cambridge Laboratories. Pons claims there are variations in performance from batch to batch.

12/1005

(c) Pons claims that all Pd rods > 1 mm diameter and < 8 mm diameter have worked. A single rod 8 mm in diameter has not shown excess heat. Rods ≤ 1 mm show about a 50% success rate. All of Pons's samples are from Johnson-Matthey. He was evasive in replying to questions about whether his samples were standard materials from Johnson-Matthey. He said that there was no special pretreatment of the rods.

(d) He was definite in saying that nothing was deliberately added to the electrolyte besides Li and D_2O.

(5) Despite Pons's comments about 100% success with rods between 1 mm and 4 mm, he had no cells in operation that showed excess heat. He attributed this fact to a recent power failure.

(6) Some mention of future use of other kinds of calorimeters was made and power supplies capable of handling larger scale experiments were pointed out by Pons. No electrodes larger than those reported in their J. Electroanal. Chem. paper were visible. No closed (regeneration) cells have been used by Pons. He declared that he plans to do experiments with them.

(7) Questions concerning Harwell experiments elicited ambiguous answers implying inconsistent to negative results. Harwell had a complete cell that had shown excess heat in Pons and Fleischmann's laboratories. Moreover, Harwell worked under guidance from Fleischmann.

(8) Tritium measurements showed that the largest amounts ever observed (during bursts) was about 2.5 times background (which was 10-20 dpm).

The visit with Wadsworth's group provided much less definitive information. They reported sizeable quantities of excess heat in big, fairly short-lived excursions. However, their data were compromised by unsatisfactory instrumental limitations and by unreasonably small changes in cell voltage during the sizable temperature changes.

During this visit Michael Salamon reported on neutron (by nuclear track techniques measuring NaOH-etchable damage in polycarbonate disks) and gamma emission on Pons electrodes that were showing excess heat at the time of measurement. Preliminary spectra showed no gammas above background in any expected energy region up to 24 MeV. A "blind" location distribution of disks discussed prior to full analysis, showed no obvious high densities beyond Poisson-cosmic ray expectation. A full report of this work would be of value to the panel since it attempts a correlation of possible fusion products to calorimetric data.

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Will Happer's comments on the visit to the University of Utah with the ERAB Cold Fusion Panel on June 2, 1989

I. The Discussions with Dr. Stanley Pons

Basic measurements. The PC computer we saw was programmed to record two measurements:

1) The voltage drop across the electrodes, from which one can infer the electrical power deposited in the cell.

2) The resistance of a thermistor, from which one can infer the cell temperature, and from a proper calibration of the system, the heat flow to the surroundings.

Were the calorimeters really calibrated? The most confidence-shaking part of the Pons work was the calibration, or lack thereof, of the calorimeter. When we visited the laboratory, we saw two baths, filled with circulating water and thermostatted at about 30 C. Each bath was designed to hold four or more electrolytic cells. The cells were supplied with three pairs of electrical leads One pair supplied the constant electrolysis current. By measuring the potential drop across this pair and multiplying by the constant current value it was possible to determine the electrical power input to the cell. A second pair of wires was used read the thermistor resistance. From supposedly well known calibration curves one could infer the cell temperature from the resistance reading. A third pair of leads could be used to supply current to the calibrating heaters. The heaters were a string of fairly high-power carbon(?) resistors connected in series within a J-shaped glass tube to keep them dry. The J-shaped tube was inserted to nearly the bottom of the electrolytic cell. I noticed that only one of the cells actually had leads attached to the calibrating resistors. The leads to which no wires were attached seemed never to have had any wires attached at all. They were straight, unscratched and unbent. *My impression is that only one of the cells could ever have been calibrated.* However, a laboratory computer was recording the voltage drops, at constant current, between the anode and cathode and the thermistor resistances from each of the cells once every five minutes. The readings were stored on diskettes I believe.

Do calibration curves exist? After leaving the laboratory, we questioned Stan on the details of his experiment and particularly on the calibration. Stan's description of the calibration was vague and confusing, at least to me. He stressed that he was doing periodic checks of the *differential* calibration of his cells. From time to time he would apply a few watts of thermal power to the cell by passing current through the calibrating resistors. Of course this calibrating power could be measured very precisely. However, the calibrating power was in addition to a substantial amount of power already dissipated in the cell by the electrolysis current. This procedure

did not seem to answer the basic question: given a measured temperature difference DT between the cell and the bath, how much heat is flowing out of the cell? Stan seemed to assume that dQ/dt = C DT where C was some constant which was independent of DT. Of course, this is "Newton's law of cooling." However, it is well known that heat flow can be a more complicated function of DT and only for small DT can one be sure that Newton's law of cooling is valid. The two most important deviations to Newton's law of cooling are from convection, for example of the air between the outer wall of the cell and the inner wall of the dewar, and thermal radiation across the vacuum space between the dewar walls. I would expect convective processes in the air and also in the heavy water to be most important.

As many people have already noted, the cells were not stirred. This means that the heat transfer depends critically on the mechanisms which counter the natural tendency of a heated fluid to stratify, with the hot fluid floating on top of the cooler fluid. Although the Utah people stoutly maintain that the bubbles are adequate to thoroughly stir the solution, other groups with similar cells report that stirring makes a substantial difference in the results. Of course, the resistive heater does not produce bubbles at all, so the Pons-Fleishman arguments about bubbles as a good stirring mechanism are questionable for the calibrating heaters. Perhaps this is why the calibrating heaters were always used while electrolytic current was going through the cell and presumably producing bubbles. It is not clear how one could do an absolute calibration with the heating resistors without some mixing mechanism which mocks up the mixing of the bubbles.

If I correctly understood the talk of the Utah metallurgists (about whom, more below) they tried to calibrate the heat flow measurements in their cells by electrolyzing ordinary water, although I was so distracted by the puzzling "excursion," that I did not get the details of their procedure.

After repeated questioning, Stan showed us his "black box," which was a viewgraph with assumed heat flow mechanisms, but he was never able to show us a calibration curve for any cell. Did a calibration curve exist at the time of our visit? The metallurgists did have a calibration curve which they readily found for us.

Dewars. For still unclear reasons, the cells which Stan showed us were contained in vacuum jacketed dewars, made in the local glass shop and evacuated and sealed on the premises. The dewars were not silvered inside so radiative heat flow across the "vacuum" gap would have been relatively high. I also have some doubts about whether the vacuum was hard enough to prevent heat transport via residual water vapor which slowly outgasses from most glass systems. There was no sign of a getter inside of the dewar. This question about the efficacy of the dewar is especially important if, as I suspect, the cells were not individually calibrated with resistive heating, and a common calibration factor was used for all of them. If I understood correctly, "higher-power" cells were not contained in dewars. I guess the dewar was used to increase the temperature rise associated with the smaller heat flow in low-power cells. However, the exact path of the heat flow is unclear. If the dewar was really insulating well, it

could well be that heat was carried by convection to the top of the cell and at least partially released to the room air, which was not as well thermostatted as the water bath.

Lack of crisp answers to questions. Time and again we received fuzzy if not evasive answers to questions. Although our group was billed as an important set of visitors, and although Allen Bard had previously explained to Stan Pons that we wanted to work through the nitty-gritty details of how one analyzed the data from a cell which was generating excess heat, no cell was operating when we arrived because of a power failure. Key pieces of data, especially that pertaining to the calibration of the calorimeter were missing.

Did we see a Potemkin Village? Our visit to Stan's laboratory left me with the feeling that we had seen the classic Potemkin village. We did not see a working cell. We did not verify the crucial calibration procedure, but instead we got "trust me" as an answer. Most of the cells seemed never to have been calibrated at all, and they also seemed to have been hurriedly thrown together to impress credulous visitors rather than to take data.

II. The Discussions with the Metallurgists

According to Vice President Brophy, a group of metallurgists, led by Dean Wadsworth, from the University of Utah had independently confirmed the generation of excess heat, most spectacularly in an "excursion."

I had a better impression of the metallurgists than I did of Stan and his people. For one thing, they immediately produced a calibration curve when we asked for one. The measured function of heat flow out versus temperature rise in the cell was not at all linear, as Pons assumed, but I had the feeling that the nonlinearity was in the wrong direction. That is, it seemed that the thermal resistance increased for larger temperature differentials rather than decreased, as one might expect from convective or radiative processes.

The "Excursion." The data on the excursion consisted of a viewgraph of the voltage at constant current across the cell electrodes and also the cell temperature as a function of time. The total elapsed time was on the order of half a day. Some time during the night there was a sudden rise in temperature of about 20 C. The cell was unattended and the experiment was terminated the next morning when someone came in to refill the cell with heavy water. As I pointed out at the time, there was almost no change (a few per cent at most) in the voltage (about 10 volts) across the cell electrodes during the excursion. Since a temperature rise of 20 C would have lowered the resistance of the electrolyte by at least some tens of percent, the cell voltage data and the temperature data are completely inconsistent, and I think the excursion must be discounted as some sort of experimental problem, for example, a loose connection in the thermocouple power supply.

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III. The Discussions with Dr. Michael Salaman

Dr. Michael Salaman gave us a very clear description of his search for gamma rays or neutrons from the Pons cells. Contrary to earlier assertions by Pons and Fleishman, there are no neutrons or gammas above the natural background. The gammas were examined with a large sodium iodide scintillator, located directly beneath one of Pons's water baths. Very low limits could be set on the 2.2 Mev capture radiation of neutrons in water and on the 24 Mev radiative capture radiation of two deuterons into the ground state of helium-4. The neutrons were detected by attaching a uranium-235 foil to a plastic (lexan?) substrate and subsequently etching out the damage track of the fission fragments. Detection sandwiches were place under each of the cells, including cells which supposedly were and were not producing excess heat. The experiments seemed very solid to me.

IV. What did we learn?

In my view it is irresponsible to say to the press that we did not "find a smoking gun." Someone in our committee has already made that statement to William Broad of the New York Times, and Broad told me today, June 6, that he was about to run a page-1 story in the Times, "Blue-Ribbon DOE Committee Endorses Cold Fusion" or some such interest catcher. I begged him not to do it, but I refused to discuss the details of what I saw. What really happened was the following:

1) There was no cold fusion in Utah on June 2 because of a "power failure" just before our visit. Once again we must accept "trust me." Sure, but let me cut the deck.

2) The purported calibration procedure of the heat flow is highly suspect.

3) Most of the cells probably were not calibrated at all.

4) The purported "excursion" in the Metallurgy Department was internally inconsistent.

5) Contrary to earlier assertions by Pons and Fleishman, there is not a trace of gamma or neutron signals near their apparatus. (Trust me, remember!)

I would be happy to endorse cold fusion if I were given the opportunity to see a working cell and verify the calibration of the heat flow measurements. I think this might take about a week of my participation in the experiments. Should the excess heat really be present and due to nuclear fusion (that is, not of chemical origin) I volunteer to join my committee members in carrying Pons in a sedan chair to Stockholm for his Nobel Prize, and I would rejoice to see him become a trillionaire for a discovery of such benefit to mankind. On the other hand, I object to whitewashing a religious cult under the aegis of ERAB.

MEMO

DATE: 6/12/89

TO: "COLD FUSION" VISITORS

FROM: JOMB

In the following, we seek to avoid misunderstandings about the work on electrochemical Cold Fusion which is going on (in three sub-groups) at Texas A&M University.

1) We are interested in the experiments reported by Fleischmann and Pons, and by Jones, which mention cold fusion obtained by electrochemical confinement.

2) We take the attitude that the presence of cold fusion in the experiments carried out by these workers is unproven.

3) Our attitude is to stress experiment. We seek to find out whether there <u>are</u> neutrons evolved from palladium electrodes under certain circumstances; whether tritium <u>is</u> produced during deuterium evolution at palladium electrodes; and whether the sometimes observed excess heat can be replicated in our laboratories.

Of course, we are interested in attempting to bring the reproducibility under better control.

4) When we have obtained reproducibility in the region of > 50%, and can instruct others how to do the experiments with the same success rate, then we shall investigate the dependence of heat evolution, neutron production and tritium evolution as a function of the variables such as overpotential, metal substrate, D/Pd ratio, dislocation density, dendritic promontories, etc.

When we have established some of these dependencies, perhaps in a year or

so, we shall then have a basis on which to decide if the New Phenomena originate in nuclear processes.

5) We are particularly unenthusiastic in the discussion of the application of present theories of fusion in plasmas to idea of fusion in electrochemical confinement because we think that the difference of conditions, particularly in respect to screening by electrons of deuterium-deuterium interaction, is an extreme one, and that it has not yet been properly investigated theoretically.

Our attitude is that we may be in an emerging area of science, and that in such situations experiment usually molds theory to fit it.

Historically, when new science is emerging, it is often reviled and denigrated until the new paradigm is accepted. It is, of course, too early to say whether this is the situation in this field.

6) At the time of writing, the phenomenon is less than three months old. Two or three years (5-6 Centers, 100 people) will be the right sort of time to think of in order to make a decision as to whether it is worth Big Money. The idea that a number of meetings are already planned, and even decisions made up on the basis of happenings at them at this time, appears to us to be unwise, partly because of the emotional outbursts by physicists which have occurred at some of them and the great negativity widely shown; but mainly because of the small degree of knowledge among us all.

Although we welcome criticism, we believe that spending a great deal of time in angry condemnation of the phenomena we are investigating is not a good way to further understanding of New Phenomena which understandablt exist. We would rather tell you in a relaxed way, about our results, and compare them with the positive results of others in various parts of the world. We believe it is agreement among scientists, particularly between those in various

countries, which eventually decides what is regarded as "truth" for a few decades in a field.

We think the new (and shaky) "facts" should be isolated from comparison with the older theories until the facts are firm and agreed upon - at least to a good degree.

8) About negative results: We think that, in attempts to verify a newly claimed phenomena, negative results have much less value than positive ones. Negative results can be obtained without skill and experience.

It has always been the anomalies which can be seen in a Science which gives rise to the new ways of thinking which cyclically invade the sciences. The constant reiteration of the old way (particularly with the great Anger and Emotion) we are seeing among our colleagues and visitors has not been the way that changes in scientific attitudes have come in the past.

Therefore, when persons tell us that they have carried out the electrolysis of deuterium evolution in palladium and see nothing new, particularly if (as is usual) they are furious about it, have spent little time on it, and have little experience as to how to do experiments of the type named, we tend to discount their contribution.

This is particularly so because the phenomena under consideration are undoubtedly elusive. Added to this is the fact that the effects - when they indeed turn on - are difficult to find in electrodes as small as 1- and 2-mm diameters (quickly chargeable), and can only easily be detected (when they display) in most calorimeters when the size of the electrode is something in the region of 4-6 mm. However, a 6-mm electrode takes 72 days to charge before the experiment can begin.

Thus, as we are now less than 72 days from the announcement, and as to start experiments it will be necessary not only to charge electrodes but to

gather equipment of various kinds both electrochemical and nuclear, -to say nothing of super-pure Pd rods, - it is remarkable that those who were not already working in electrochemistry before the announcement was made could have made experiments at all, let alone gotten results upon which the National Policy (in funding) is to be founded.

Most of the experiments in which negative results have been obtained have come from Laboratories which have little record of research in physical electrochemistry; or, when in a tiny number of cases the laboratories were electrochemical, little experience in nuclear measurements.

The most common errors to date are:

a) Insufficient charging times. The latter is obtained from the use of the formula $\Delta^2 = 2DT$. Our habit is to calculate using this formula for α and β Pd (hence, 2 T's), and the D is for the two forms of palladium, then double the time to allow for possibility of a third form which seems to exist. (We get c. 30 days for 4 mms and 72 days for 6).

b) The second most common error is the use of electrodes less than 4 mm in diameter.

There is, at first, an apparent advantage to using these because they charge up more quickly. The disadvantage is that the effects they give are often too small to see: they need a micro-calorimeter.

c) Contact with the wet atmosphere eliminates the observation of the heat. Water must be excluded from the D_2O.

d) Use of inappropriate palladium is difficult to specify. There is some evidence is that Johnson-Matthey's "puratronic" palladium works best, but we do not know why, and the evidence is conflicting. Annealing, hammering, cold work, casting, have all been suggested.

e) The final experiment in which the phenomena are sought should be carried out at more than 500 ma/cm^{-2}. After the charging time of 30 days for the 4-mm rods and 72 days for the 6-mm rods, there has to be a further time up to 500 ma/cm^{-2} which may last several days before anything abnormal is seen.

We do not give up an electrode until 7 days after the current density has been turned up to 500 ma cm^{-2}.

f) Much confusion and waste of money is carried out by examination of electrodes which have never "woken up".

When an electrode doesn't show the heat, there is little point in examining it in great detail with neutron counters ("the most sensitive in the world") or etc., or X-ray monitors. If it does not show heat, it is less likely to produce tritium, etc.

Reports are full of accounts of people who did this and spent time seeking nuclear particles and not finding them (no wonder, if the electrode did not give heat).

g) There has been too much accent on very accurate calorimetry. Our experience is that when the effect switches on, it switches on very definitely, and using the size of rods mentioned above, calorimeters which are only measured to ~100 milliwatts can easily measure the effects.

h) Keeping water out: The separation factor of water to deuterium is 9 times in favor of water, so that a 0.5% water-containing solution will evolve about 5% hydrogen. Small amounts of hydrogen seem to poison the electrode.

It's probably better to keep the water below 0.1%.

i) The use of LiOH instead of LiOD: Remarkably, a number of laboratories have used LiOH - not good for obvious reasons.

j) Lack of preelectrolysis of the solution: The latter is very

necessary, for it removes the water and takes away other undesirable impurities.

k) Lack of knowledge of the Tafel parameters

Exchange current density?

Overpotential?

D/Pd ratio?

We have found that these vital basic elements are understandably little known to physicists working on fusion. It's vital to know them because the fugacity developed in the electrode depends upon the detailed relationship between the Tafel slope and the overpotential, - and then depends on the relevant mechanism of deuterium evolution, intermediate concentration, etc.

l) Neutron measurements: Arrangements for screening out cosmic ray showers are, of course, essential. Neutrons as a function of the <u>state of the surface</u> are informative.

m) Tritium: Tests for the elimination of chemiluminescence is essential. The plot of the tritium build up in the solution as a function of time may be informative. Conversely, we don't always find tritium when we find heat.

Finally, there is no doubt that irreproducability is the bane of these experiments. We are looking increasingly towards the concept that the phenomenon occus at the surface rather than in the interior, although of course the state of internal saturation will effect the surface concentration of intermediate deuterons.

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Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 12, 1989

TO: Cold Fusion Panel

The following materials are circulated for your information.

1. Memorandum from Darleane Hoffman with attachment.
2. Report by Jacob Bigeleisen.
3. Logistical information for those visiting CALTECH.
4. Paper from J. O'M Brockr's, Texas A&M.

B. II
William Woodard

4 - 765 WE

...g-splitting of Landau levels ... po... ... more detailed analysis of the struc... reported here for stimulated-emission spectra of a hole plasma in germanium in crossed electric and magnetic fields.

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Translated by Dave Parsons

High-energy processes accompanying the fracture of solids

V. A. Klyuev, A. G. Lipson, Yu. P. Toporov, B. V. Deryagin, V. I. Lushchikov, A. V. Strelkov, and E. P. Shabalin

(Submitted January 9, 1986)

Pis'ma Zh. Tekh. Fiz. 12, 1333–1337 (November 12, 1986)

It is well known that the fracture of solids and the breaking of adhesive bonds at a contact are accompanied by many interrelated electrophysical processes.[1,2] It has been found from research on these events that the solid surfaces which are formed in the course of the fracture are electrically charged, and the electric field between the walls of a propagating crack is on the order of 10^6–10^7 V/cm. As a result, significant accelerating voltages (100 kV and above) are produced in the fracture zone, creating conditions for the acceleration of electrons emitted from the newly formed surface up to energies of 100 keV (Refs. 2 and 3).

Recent studies on the fracture of certain solids containing hydrogen have revealed the evolution of atomic hydrogen at a flux density up to 10^{14} cm^{-2} (Ref. 4). Simultaneous studies of the emission of positive ions and electrons from the fracture zone have shown that these particles are emitted simultaneously, in roughly the same numbers, and that their energies are essentially the same.[5]

The intense electric fields and the free heavy particles and electrons cause intense gas-discharge processes in the fracture zone. The processes are similar in nature and in their properties to spark discharges.[1] This similarity has been confirmed by the emission of rf waves with very broad frequency spectrum (up to tens of megahertz) and x radiation.[2] One characteristic feature of the x radiation which has been observed is that the frequency spectrum of the bremsstrahlung has an exponential dependence and contains x rays with energies ≫50 keV. In several cases involving the fracture of certain solids by elastic waves from an explosion, the energy of the emitted x rays can reach 4 MeV (Ref. 6).

These results are evidence of the appearance of small regions with a high energy density in the zone where a solid fractures. The intense electric field in these regions creates conditions for significant acceleration processes. The high density of charged particles suggests the presence of local plasma formations in these regions.[2]

Since the propagating crack has strong electric fields and high charged-particle fluxes, and since the electric field in a crack increases with increasing velocity of the crack,[1] it may be possible that if certain fracture conditions are obtained and if the material to be fractured is chosen appropriately particles can be accelerated in the zone between the separating surfaces of the walls of a propagating crack and as a consequence of this acceleration, nuclear reactions which do not require high energies, or photonuclear reactions in low-threshold materials might occur.

It was assumed,[4] for example, that, as a deuterium-containing material is fractured, regions with a high energy density are produced in the fracture zone, or as a result of acceleration processes the walls of a crack are bombarded with a stream of accelerated deuterons, then it might be possible to observe the emission of neutrons from the reaction D(d, n)He[3]. This reaction is highly exothermic and can occur in principle at any deuteron energy. However, because of the Coulomb barrier, a significant yield of neutrons can be expected only at deuteron energies beginning at a few kiloelectron volts. At a deuteron energy of 10 keV, for example, the cross section for the reaction is[8] 10^{-6} b, while the yield of neutrons is on the order of 10^{-11} per deuteron.

To test this suggestion we carried out some experiments designed to detect neutron emission during the impact fracture of LiD single crystals. This particular material was chosen because lithium deuteride has a structure analogous to that of alkali halide crystals, which become highly charged upon fracture. Furthermore, deuterium ions may be desorbed directly from the crystal lattice into the volume of the crack.

The experimental apparatus is shown schematically in Fig. 1. The LiD crystals are fractured by a metal

6-765 WE

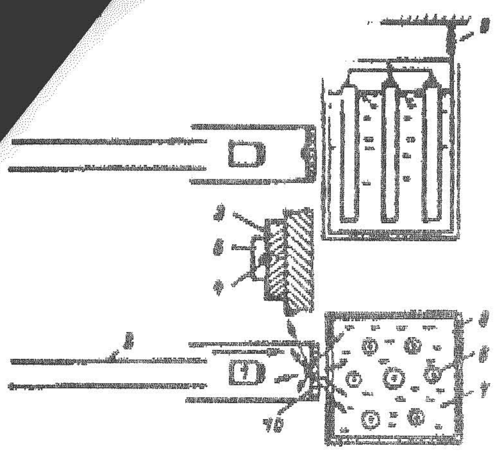

FIG. 1. The experimental apparatus.

striker with a mass of $5 \cdot 10^{-2}$ kg (1), which is accelerated in the barrel of an air gun (2) to a velocity of about 200 m/s. The target is a lead plate (3), 7 mm thick, on one side of which is a conical depression 5 mm deep. The LiD single crystal (4) is placed in this depression. This crystal is cubic in shape with a side of 3–4 mm. The crystal is covered with a thin brass cover (5). The target is mounted on the vertical wall of a chamber (10).

Neutrons are detected with an array of seven NWI-62 proportional counters (6), immersed in an oil-filled tank (7) whose walls are coated with sheet cadmium (8). To avoid acoustic interference, the tank is mounted on a spring suspension (9) in the immediate vicinity of the chamber holding the target. The results are fed to an AI-256-6 pulse-height analyzer with digital printout and an S8-17 storage oscilloscope. The analyzer is turned on for a time ~1 s. The shot is fired at the middle of this time interval. The efficiency of the detector was evaluated with a P_0-α-Be neutron source with an intensity of 200 n/s, positioned in place of the target. At certain time intervals we measured the natural neutron background. To check the reliability of the experimental data, we also measured the background during shots at targets not containing LiD crystals (the pulsed background").

The results of the measurements are summarized in Fig. 2a-d as histograms of the distribution of counts over the channels of the analyzer. Measurements with the neutron source (Fig. 2a) show that most of the neutrons are detected by this apparatus in channels 12–32 of the analyzer, which correspond to the energy of the reaction B^{10} (n, α)Li^7.

The experimental results show that the neutron count during shots at the LiD target exceeds the pulsed background. The corresponding values, averaged over 75 shots, are 0.42 ± 0.09 count/shot and 0.16 ± 0.06 count/shot (we have subtracted the cosmic-ray neutron background, which is 0.11 count/s or 0.18 count/shot).

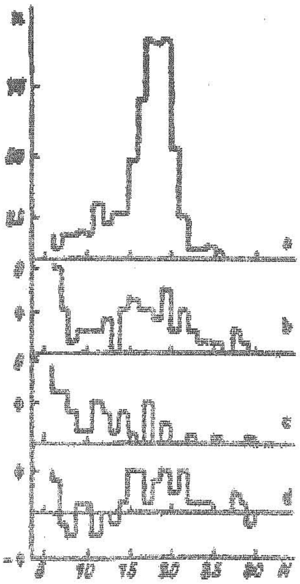

FIG. 2. Histograms of the distribution of counts over the channels of the pulse-height analyzer. a) From a neutron source; b) during the fracture of lithium deuteride targets; c) during a shot on a target not containing a lithium deuteride crystal (the "pulsed background"); d) result of a subtraction of the pulsed background and of the cosmic-ray background from the pulse-height spectrum measured during the fracture of lithium deuteride targets.

Figure 2d shows the results of a subtraction of the pulsed background and of the cosmic-ray background from the pulse-height spectrum measured during the fracture of LiD targets. The suggested effect – neutron emission – is 0.26 ± 0.11 count/shot. Taking the efficiency of the neutron detector into account, we conclude that something on the order of ten neutrons are emitted per fracture of the lithium deuteride single crystal.

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Translated by Dave Parsons

7- 765WE

Lawrence Berkeley Laboratory
1 Cyclotron Road Berkeley, California 94720
(415) 486-4000 • FTS 451-4000

June 5, 1989

TO: Norman Ramsey, Co-Chairman
John Huizenga, Co-Chairman

FROM: Darleane C. Hoffman

On Friday, May 26th, I visited at LANL and saw some of the experiments set up in an attempt to observe neutrons from electrolysis and from Ti treated with high pressure D gas. In addition, Malcolm Fowler et al. (INC-11) are setting up an experiment to try to determine whether electrons are emitted in coincidence with the bursts of n's generated when deuterated Ti and other materials are allowed to warm from liquid nitrogen to room temperature. This would attempt to check the fracture hypothesis.

To my mind, the most credible n measurements to date are those observed at LANL in Howard Menlove's neutron detection system with 34% efficiency. I believe we should either visit these facilities at LANL, or, probably a more expedient alternative, invite both Dr. Menlove and Dr. Fowler to brief the Committee at its June 22nd meeting in Washington, D.C. Their results are very low level, but scientifically interesting and important as an explanation of the "Jones" effect. (See attached reprint on the generation of n's from fracturing of LiD and subsequent D-D fussion ($\sigma \cong 10^{-5}$ b at $E_D \cong 10$ keV and yield of n's $\cong 10^{-11}$ per D.) It is important to determine if a similar mechanism can account for the bursts of n's observed in the LANL experiments. Thus, the experiment Dr. Fowler is setting up to determine if e⁻s are in coincidence with n's is especially important. This mechanism would certainly not explain the large heat effect claimed by Pons and Fleischman but might be consistent with the initial report of Jones (although more convincing than Jones' initial reports because of the LANL n-detection efficiency of ~ 36%). I think the Committee should evaluate these LANL experiments, which appear to be the most credible (if not the only credible) positive neutron results. They are much lower than the Frascatti (Italy) results which appear to be an artifact.

cc: Bill Woodard

5-765 WE

OBSERVATION OF TRITIUM PRODUCTION DURING ELECTROLYSIS OF HEAVY WATER SAMPLES USING PALLADIUM WIRES.

N.J.C. Packham, K.L. Wolf, M.E. McLain and J.O'M. Bockris

Department of Chemistry, The Cyclotron Institute, and Department of Nuclear Engineering, Texas A&M University, College Station, Texas 77843

INTRODUCTION

A research group here has been studying an effect described by Pons and Fleischmann [1]. Research has centered around the investigation of the metallurgy of the palladium, anomalous heat production and the detection of nuclear emissions. Here, we describe the observation of tritium in seven out of eleven electrochemical cells, at levels which could not be produced by any process other than a nuclear one.

EXPERIMENTAL

The electrochemical cells used were 15 ml Pyrex centrifuge tubes, sealed with Viton rubber septa. Palladium samples supplied by the Texas Coin Exchange, 1 mm by 4 cm, and 3mm by 4 cm in dimension (99.9% purity) were prepared as shown in Table 1. All electrode connections were made using 99.9% pure nickel wire (0.5 mm). Nickel gauze anodes were used in all cases (99.9% purity). Electrode connections were fed through the rubber septum to the outside. 0.1M LiOD was prepared using 99.9% pure lithium metal from Alfa Associates, added to 99.9% pure deuterium oxide (Aldrich Chemical Co.), in an atmospheric bag containing argon. Additions to the cell (such as D_2O refilling) were performed by using disposable syringes (one use only), equipped with stainless steel needles. Gases evolved during electrolysis were

allowed to escape through a needle attached to Tygon tubing to a mineral oil bath to avoid light water contamination. All 1mm cells were at first run at 60 $mAcm^{-2}$ for 14 to 16 days, the 3mm electrodes being charged for up to 28 days. After this time, the current density was increased to 500 $mAcm^{-2}$ for periods of up to 8 hours. The cells were monitored electrochemically continuously during this time. Samples of electrolyte were withdrawn from the cell using a sterile syringe (later discarded).

Liquid Scintillation Counting (LSC) was performed using a LKB-Wallac Model 1219 Rackbeta LSC. A water soluble scintillation cocktail (Biosafe II, Research Products International Corporation) was added to 1 ml of sample. After allowing time for deexcitation of the cocktail, the samples were run in a double blind fashion. Blank samples of H_2O, D_2O, and 0.1M LiOD were also included for analysis. The efficiency of the detector for tritium was 33%. Analysis of samples was also confirmed in another instrument at this institution, and additionally by one private and three National laboratories.

RESULTS AND DISCUSSION

The activity of the samples in disintegrations per minute per ml of solution are shown in Table 1. In one of the cells (designated A7) the build up of tritium as a function of time was followed at high current density, and the results are shown in Fig. 1. The possibility of chemiluminescence of the scintillation cocktail at 0.1M LiOD was ruled out by the experimental study shown in Fig. 2.

If tritium is produced only at high current density (cf. Fig. 1), based on the observed rate of tritium production, calculation shows that in the highest activity sample (cell A3), approximately 10^{10} atoms of tritium are produced per second, neglecting losses in the gas phase (which may be

appreciable).

If tritium is produced according to the following equation:

$$^{2}_{H} + ^{2}_{H} \longrightarrow ^{3}_{H} + p + 4.02\ MeV \qquad (1)$$

, at 10^{10} atoms per second this would be equivalent to 2 milliwatts, not detectable in the calorimeter at present in use in this laboratory. Cells A4 and B5 have also shown emissions of up to 50 neutrons per minute which will be described in detail elsewhere.

Although it has not been proved that the electrodes which produced the tritium reported here also produced the Fleischmann-Pons heat (approximately 10 Watts cm^{-3}), palladium electrodes prepared in a similar way did so. The ratio of total heat production to that accounted for by the tritium in solution, 5×10^{3}, is a reasonable ratio (at 1 $Ampcm^{-2}$) for the gas (DT) [illegible] figure 1 is consistent with the establishment of a quantity of DT in the electrolyte in equilibrium with DT in the escaping DT-D_2 mixture.

A problem is the sporadicity of the effect*. It is noteworthy that nickel anodes were used in the charging of electrodes which gave tritium. An alternative explanation may concern the times of charging of these electrodes. Negative results have been reported mainly for larger (>2 mm) electrodes which might not yet be ready for high current density treatment until times approaching 2 months from initiation of charging.

One may speculate that tritium only forms in an electrochemical surface reaction when dendritic growths having low radii of curvature ($<10^{-5}$cm) have

---

* But cf. Schoessow and Wethington [2] who have obtained 50,000 disintegrations per minute per ml.

formed on the surface**. Here, at the low radius of curvature tips, local electric fields of approximately 10^{10} Volts cm^{-1} may bring the D^+ ion in transfer to an energy of 10 keV*** sufficient to fuse with an adsorbed $D^{\delta+}$ on the electrode surface. The region of the electric double layer at the metal surface is known to be electron-rich [3] and thus provide the screening which may allow a D^+ having an energy in the 10 keV range to fuse with a surface D^+.

ACKNOWLEDGMENTS

We acknowledge the financial assistance of the following; The Welch Foundation, the Electric Power Research Institute and Texas A&M University. We gratefully acknowledge the assistance of the following; Ross Lemmons, Bob Sherman and Roland Jalvert of Los Alamos National Laboratory, Dave Robertson and Russ Jones of Battelle, Pacific Northwest Laboratory, Kevin Myles of Argonne National Laboratory, Dennis Corrigan of General Motors Research Laboratory, and the following from Texas A&M University: Ramesh Kainthla, Omo Velev, Jeff Wass, Lamine Kaba, Guang Lin, Marek Szklarczyk, Arpad Szucs, Babla Kapur, Maria Gamboa-Aldeco, Anuncia Gonzales-Martin, Ljiljana and Zoran Minevski, Jeng King-Tsai, Yang Bo, Lin Chen, Peter Lee, and John Shoemaker.

REFERENCES

1 M. Fleischmann and S. Pons, J. Electroanal. Chem., 261 (1989) 301.

2 G.J. Schoessow and J.A. Wethington, private communication, 1989.

3 W. Schmickler and D. Henderson, J. Chem. Phys., 85 (1986) 1.

** Scanning Electron Micrographs of the surface of electrodes prepared in a similar way to ours have shown dendritic growths

*** Equivalent to a temperature of approximately 10^8 ^oK

FIGURE CAPTIONS

Figure 1. The production of tritium in the electrolyte of cell A7 (see Table 1) as a function of time.

Figure 2. The effect of electrolyte concentration on chemiluminescence of the scintillation cocktail.

TABLE 1
Cell identification, electrode treatment, solution type and tritium activity of electrolyte samples.

CELL	ELECTRODE TREATMENT[a]	SOLUTION[b]	ACTIVITY (d $min^{-1}ml^{-1}$)
A1	A	1	3.8×10^4
A2	A	2	315
A3	B	1	4.9×10^6
[c]A4	B	2	1.2×10^5
A5	C	1	3.7×10^6
A6	C	2	3.3×10^4
A7	D	1	
	Before high current density		249
A7	After 2 hours at 500 $mAcm^{-2}$		5370
A7	After 6 hours at 500 $mAcm^{-2}$		5.0×10^5
A7	After 12 hours at 500 $mAcm^{-2}$		7.6×10^5
A8	D	2	339
B3 (3mm)	B	1	6.3×10^4
B5 (3mm)	C	1	195
CELL 1 (6mm)	A	1	264
D_2O			195
0.1M LiOD			225
Neutralized 0.1M LiOD			220
Neutralized 0.1M LiOD + 0.1mM NaCN			230

[a] Key for electrode surface pre-treatment: (A) No surface pre-treatment; (B) anneal 800 °C, 10^{-6} torr, 8 hours; (C) acid etch, 5M HCl, 15 minutes; (D) electrochemical oxide removal, 2 hours

[b] Key for solution type: (1) 0.1M LiOD; (2) 0.1M LiOD + 0.1mM NaCN

[c] Cell that has shown neutron activity up to 50 neutrons per minute.

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TIME PROFILE OF TRITIUM PRODUCTION FROM CELL A7

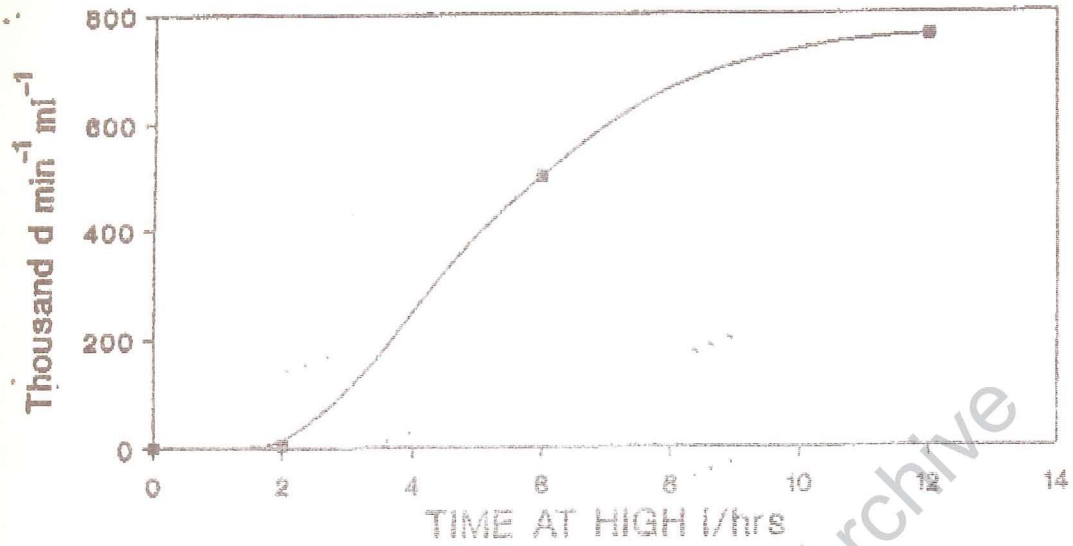

Figure 1 Packham et al Tritium Reduction 23-765WE

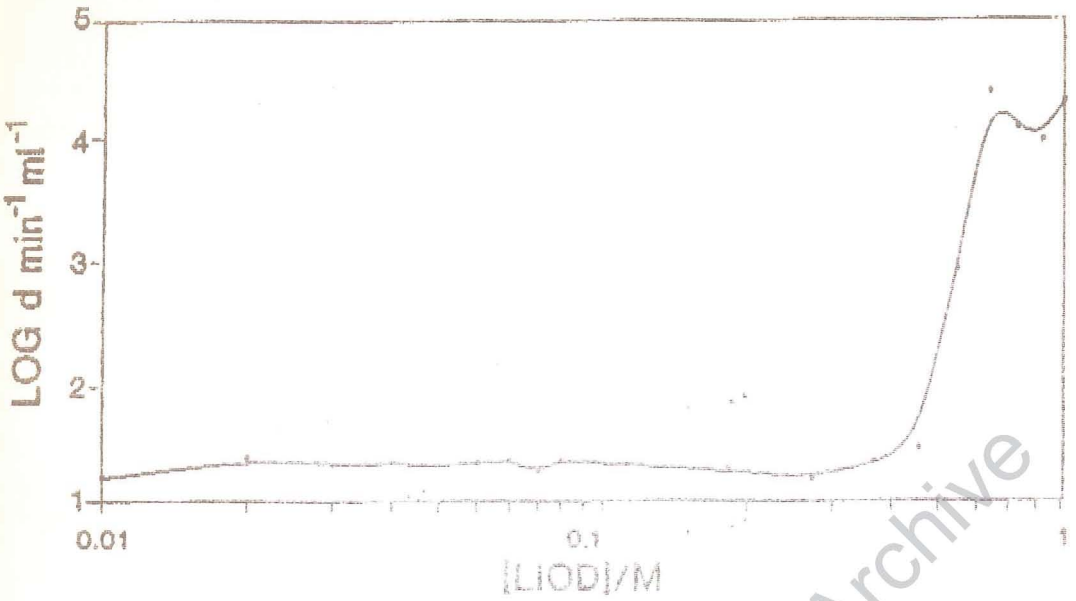

Figure 2 Packham et al. Tritium Retention

21-765 WE

WHAT'S NEW, Friday, 9 June 1989 Washington, DC

1. FANG LIZHI FOUND ASYLUM IN THE AMERICAN EMBASSY IN BEIJING on Monday, along with his wife Li Shuxian, a professor of physics. Fang, a prominent astrophysicist, is the spiritual father of the student movement toward democracy in China. Fang had reportedly eluded arrest by constantly moving until he reached the embassy. Chinese authorities are furious at the US for giving Fang refuge.

2. THE STATUS OF PRC CITIZENS IN THE U.S. WILL BE "LIBERALIZED" in light of events in the People's Republic of China, according to a statement issued by President Bush on Monday morning. Later that day, the Congressional Human Rights Caucus wrote to Attorney General Thornburgh requesting clarification. The letter, which was signed by the co-chairs of the Caucus, Reps. Lantos (CA) and Porter (IL), specifically asked for guidelines for PRC students in the US on J-1 "exchange visitor" visas. The Administration later announced that J-1 visas would be extended for one year. There are currently about 37,000 Chinese students in the US.

3. THE NATIONAL ACADEMY OF SCIENCES SUSPENDS EXCHANGE WITH CHINA. A message to Zhou Guangzhao, President of the Chinese Academy of Sciences said, "We are shocked and dismayed by the action of the Chinese government troops against peaceful demonstrators in Tiananmen Square and elsewhere in Beijing, with such great loss of life. While we earnestly hope to maintain our cooperation with your Academy and other Chinese institutions, we must suspend all activities for the time being. We do so in outrage and sadness."

4. IS THERE HELIUM IN THE PONS-FLEISCHMANN CATHODES? We had expected to reveal the dramatic results of the Johnson-Matthey analysis today (WN 2 Jun 89). The analysis was delivered to Pons this week, but he has decided not to make it public until they publish a paper sometime, according to James Brophy, Research VP at Utah. In the meantime the tension will be almost unbearable.

5. PONS DENIED THE DOE'S COLD FUSION PANEL ACCESS TO HIS LAB last week (WN 2 Jun 89) until he got the OK from the University of Utah's lawyers. In a university run by lawyers, intellectual property is no doubt well protected, but it must be difficult to know whether it's worth protecting. The panel also intends to visit Texas A&M, Stanford, Brigham Young and Cal Tech. Other labs, including national labs, will simply be contacted by phone.

6. A GAO INVESTIGATION OF LIVERMORE IS ITSELF UNDER INVESTIGATION by a House Subcommittee chaired by John Dingall (D-MI). Last year a General Accounting Office report (WN 22 Jul 88) seemed to clear the lab of charges that the x-ray laser was oversold. The x-ray laser has since faded into oblivion, which says something about the GAO report, but the curiosity of the subcommittee was aroused by the discovery that one of the three GAO auditors had been hired by Livermore just three months after release of the report.

Robert L. Park (202) 232-0189 The American Physical Society

6N

-R.L. GARWIN-

13 JUN 89 13:45

Energy Research Advisory Board
to the
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1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 7, 1989

TO: Members of the Cold Fusion Panel

SUBJECT: Agenda for June 19 Meeting at Texas A&M University

For your information.

Bill

William L. Woodard
Panel Secretary
(202) 586-5767

Enclosure

060789.WLW

PRELIMINARY ARRANGEMENTS AND AGENDA FOR TEXAS A&M UNIVERSITY

COLD FUSION TASK FORCE REVIEW
JUNE 19, 1989

DOE Representative in Charge: William Woodard - Tom Finn

TAMU Cyclotron Institute: (409) 845-1411, FAX (409) 845-1899
Contact: K.L. Wolf or Karin Allen

AGENDA

June 18	Arrival Easterwood Airport - Hampton House van to hotel
June 19	
8:15 a.m.	Hampton House Shuttle to Cyclotron Institute
8:30	Overview of Effort - Kenneth Hall, Deputy Director, Texas Engineering Experiment Station
8:45	Precision Calorimetric Measurements - John Appleby and S. Sirinivasan, Engineering Research Center
9:35	Nuclear Reaction Products - Kevin Wolf, Cyclotron Institute and the Department of Chemistry
10:15	BREAK
10:25	Tour Cyclotron
10:45	Results and Chemical Explanations - John Bockris and Nigel Packham, Department of Chemistry
11:35	Results from Calorimetric Measurements - Charles Martin, Department of Chemistry, Kenneth Marsh, Bruce Gammon, Texas Engineering Experiment Station
12:05	LUNCH - Cyclotron - Closed Committee Session
1:05	Tour Chemistry - Bockris, Martin Labs
1:55	Tour ERC Calorimeter Lab
2:25	Tour Marsh, Gammon Calorimeter Lab, McClain Tritium Counting Facility
3:00	Final Discussion, Cyclotron Institute
3:50	To Airport for 4:30 Departure

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 6, 1989

TO: Cold Fusion Panel

SUBJECT: Visit to Texas A&M (June 19) and CALTECH (June 20)

TEXAS A&M

Those who have indicated that they are attending are: Allen Bard, Jacob Bigeleisen, Clayton Callis, John Huizenga, Barry Miller, Norman Ramsey, John Schiffer, Mark Wrighton, and possibly Richard Garwin.

Reservations have been made for participants at the Hampton Inn in College Station. Their address is 320 Texas Avenue South, and their telephone number if (409) 846-0184. The rate is $35.00. If you will be arriving after 6:00 p.m., please call them with a credit card number to confirm your reservation. A block of rooms has been reserved under "Cold Fusion Task Force". When you call them, if you let them know the time your flight arrives at College Station they will have a courtesy van pick you up. The Inn will also provide transportation to the University on the 19th. Let the driver know that you want to be picked up at the Inn at 8:15 a.m. and taken to the Cyclotron Institute at University and Spence Streets.

On your arrival at the Cyclotron Institute at 8:30 a.m., go to the receptionist who will direct you to the conference room where the meeting will begin. Most probably you will be met at the reception area by your host, Dr. Kevin Wolfe. His telephone number, if you should need it, is (409) 845-1411. The meeting and tours will end about 4:00 p.m. in plenty of time for you to return to the airport for those who are going to Burbank on the 4:30 flight.

CALTECH

Those who have indicated they are going are: Allen Bard, Jacob Bigeleisen, John Huizenga, Barry Miller, Norman Ramsey, and Mark Wrighton.

Reservations have been made for participants at the Pasadena Hilton on 150 South Los Robles. Your rooms have been confirmed for the night of the 19th but you should call and given them your credit card number to guarantee for a late arrival. Their number is (818) 577-1000. The rate is $72. The Hotel has courtesy vans to take you from the airport to the Hotel.

Your meeting will begin at 8:30 a.m. and will start in the office of your host for the meeting, Nate Lewis. His office, which is about five minutes from your hotel, is in Room 210 of the Noyes Building on the CALTECH campus. The Noyes Building is near the corner of San Pasqual and Wilson. I will send you

a campus map as soon as I get it. Dr. Lewis suggests you contact him and let him know what specific items or questions you may have. His telephone number if (818) 356-6355. If you cannot reach Dr. Lewis, you can leave a message with his Secretary, Pat Anderson. When you know what your departure time on the 20th is, Pat will also help you reserve transportation to the airport. You should call her with this information by no later than June 16.

Other members who wish to go on these site visits should let me know as soon as possible.

For all members who are making these visits, as soon as you have selected your flights, please let me or Sarah Goldman know so that we can have your tickets issued. Sarah's telephone number if (202) 586-5779.

As I mentioned last week, we were not able to schedule a visit to Stanford University on either June 20 or 21st. Dr. Huggins is available on July 6 or 7 and I would appreciate it if you would let me know if you are interested in making this trip. One of the two days will then be selected by the Co-Chairmen.

Bill

William L. Woodard
Panel Secretary
(202) 586-5767

cc: Kevin Wolfe, Texas A&T
Nate Lewis, CALTECH

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

June 6, 1989

Dr. Douglas R.O. Morrison
CERN Laboratoire 1
1211 Geneve 23
SWITZERLAND

Dear Douglas,

It was fine to see you at CERN. Thanks for your help during my talk.

I had mentioned to you my experiments to detect gravity waves, and I think you commented in Santa Fe that I had "gone on too long" in that activity. So long as you are working on pathological science, you might want to have the documentation.

Here is not all of Weber's publications, but all of mine on gravity waves. In addition, of course, I have extensive correspondence with Weber and others, but what is most important is what is published.

Doug, I am glad that we were able finally to communicate via BITNET. It will be easier in the future.

Sincerely yours,

Richard L. Garwin

Encl:

11/00/75 "More On Gravity Waves," Physics Today, p. 13. (110075.MGW)

05/20/75 "Electronics for the IBM Gravity Wave Detector-- Concept, Implementation, and Experience," Proceedings 2nd ISPRA Nuclear Electronics Symposium, book, (May 20-23, 1975), pp. 77-83, (with J.L. Levine). Publisher: The Commission of the European Communities, Luxembourg, 1975. (052075EIGW)

12/00/74 "Detection of Gravity Waves Challenged," Physics Today, p. 9-11. (120074DGWC)

09/23/74 "New Negative Results for Gravitational Wave Detection, and Comparison with Reported Detection," Physical Review Letters 33, No. 13, pp. 794-797, (with J.L. Levine). (092374NNRG)

06/10/74 "The Evidence for Detection of Kilohertz Gravitational Radiation," presented at the "Fifth Cambridge" Conference on Relativity (MIT). (061074EDKG)

07/16/73 "Absence of Gravity-Wave Signals in a Bar at 1695 Hz," Physical Review Letters 31, No. 3, pp. 173-176, (with J.L. Levine). (071673AGWS)

07/16/73 "Single Gravity-Wave Detector Results Contrasted with Previous Coincidence Detections," Physical Review Letters 31, No. 3, pp. 176-180, (with J.L. Levine). (071673SGWD)

08/15/76 "Gravitational-Radiation-Detector Observations in 1973 and 1974," by M. Lee and D. Gretz, Physical Review D, 14, No. 4. (081576..ML)

09/28/74 "Comment on a Publication by Bramanti, Maischberger and Parkinson on Gravity Wave Detection," written by J.L. Levine. Lettere al Nuovo Cimento, II, No. 4. (092874.JLL)

RLG:jah:157%DROM:060689DROM

Energy Research Advisory Board
to the
United States Department of Energy
1000 Independence Avenue, S.W.
Washington, D.C. 20585
(202) 586-5444

June 5, 1989

To: Cold Fusion Panel Members

Subject: June 22 Meeting

The Panel meeting will be held in Room 4A110 of the Forrestal Building, 1000 Independence Avenue, SW. It will begin at 9:00 a.m. and coffee will be available starting at 8:30. Lunch will be in Room 6A110 and will cost about $5.00.

As is the usual practice for Panel meetings, Sarah Goldman will send you "OPEN" tickets and you will then be able to make the reservations of your choice. Normally we do not pay for the travel of Panel members who are employees of National Laboratories, but rather expect the Laboratories to pick up these costs. Therefore, Sarah will not be issuing you tickets unless you specifically request them. I am also enclosing a listing of hotels in Washington, DC and I urge you to make your reservations early.

Bill

William L. Woodard
Panel Secretary
(202) 586-5767

Enclosure

050589.WLW

AGENDA

1. Review of Secretary's charge letter

   - Interim report by July 31
   - Final by November 15
   - Scope of Study

2. Organization of the Panel

   - Do we want Subpanels? If so, members should be designated, with a specific charge for each group

3. Background Information

   - What data does the Technical Advisor need to collect and distribute?

4. Site visits

   RLG visit Fraser's before Exxon?

   - Where and by whom?

5. Agendas for June 22 and July 11-12 meetings

   - Does the Panel want to invite any guest speakers?
   - Are there any gaps in the Panel's group expertise which should be supplemented?

6. Schedule beyond July meeting

   - Any additional meetings required?

7. Draft report

   - How best to structure?
   - Who will draft what?

Cold fusion shelf/hrafte

Received: from CEARN by CEARN.cern.ch (Mailer R2.07B) with BSMTP id 2063; Tue, 03 Jul 90 15:13:58 GVA
Received: from dxmint.cern.ch by CEARN.cern.ch (IBM VM SMTP R1.2.2MX) with TCP; Tue, 03 Jul 90 15:13:35 GVA
Received: by dxmint.cern.ch (cernvax) (5.57/3.14)
id AA14807; Tue, 3 Jul 90 15:10:54 +0200
Message-Id: <9007031310.AA14807@dxmint.cern.ch>
Date: Tue, 3 Jul 90 15:13 GMT +1
From: MORRISON%VXPRIX.decnet.CERN@cernvax
Subject: Cold Fusion Update.
To: rlg2@yktvmv
X-Vms-To: MINT::"rlg2@yktvmv"

Dear E632 and WA84 Colleague, 23 June - 2 July 1990.

COLD FUSION UPDATE

TRITIUM RESULTS EVAPORATE - POSSIBLE FRAUD?

The 23 Cold Fusion News letters that I distributed were meant to be essentially scientific though they gave some news as well. As there are so many good null experiments and so few good experiments that find positive results favouring Cold Fusion, I decided that there was no scientific point in continuing. However surprisingly many people ask me what is happening, and since there have been some dramatic developments casting very serious doubts on the tritium experiments, which Believers consider to be their strongest evidence, this is an update.

Hence this Update is not intended to be conventionally scientific. It may concern those interested in Pathological Science: also senior scientists and scientific directors since there is growing activity among US politicians about scientific fraud and misuse of funds - e.g. the National Science Foundation started an Inspector General's office a year ago and now has appointed a criminal investigator. Fraud is very, very rare (in my own field of Particle Physics the most recent example I know of was in 1924 when the positron was "discovered"). The result is that often scientific administrators react badly throwing buckets of whitewash over their colleagues but acid at the honest whistle blower and the media - whereas they could be starting an immediate investigation which they can announce if challenged.

Will those who do not wish to receive an Update please let me know.

SUMMARY

Kevin Wolf of the Cyclotron lab at Texas A&M has announced that there was contamination of tritium in the palladium rods he used and this could explain the tritium that he had previously claimed.

Many scientists at Texas A&M had been worried about the possibility of fraudulant addition of tritium to the samples of electrolyte sent for analysis from Dr. Bockris's lab and which gave exceedingly high levels of tritium. There was a bottle containing tritium in light water in the lab. The samples were found to contain light water. Despite repeated requests, Dr. Bockris did not take adequate precautions to prevent someone spiking the samples.

The Senate of the University of Utah has asked the President to resign. He says he intends to leave next year.

Experiments and conferences on Cold Fusion continue.

SUBJECTS

1. Tritium in Original Palladium Rods

2. Possible Fraud in Dr. Bockris's lab.

3. Other Tritium Claims

4. University of Utah Tries to Cleanse Itself

5. Cold Fusion Experiment in Kamiokande Detector

6. Future Conferences on Cold Fusion and Anonomolous Effects.

7. When did Cold Fusion begin?

8. Problems in Dealing with Fraud.

1. TRITIUM IN ORIGINAL PALLADIUM RODS

At Texas A&M there are four groups that have worked on Cold Fusion, those of Kevin Wolf, Dr. Bockris, Dr. Appleby and Dr. Martin. The Wall Street Journal has reported that Kevin Wolf who is a nuclear chemist working at the Cyclotron lab, might have an explanation of the small amounts of tritium he had observed from two of his cells. This was out of dozens of experiments he had run and he was currently operating 100 cells He had been attempting to explain why he found so little tritium and so rarely compared with his neighbour, Dr. Bockris who frequently found enormous amounts. One test was to dissolve completely palladium rods as received from the manufacturer, Hoover and Strong, and those used in light water blank cells. He found low levels of tritium contamination in both cases. He considered this tritium in the original palladium rod might be an explanation of his occasional findings of small amounts of tritium in the electrolyte.

Dr. Bockris said he was startled but as the level was so much lower than his labs' observations "I haven't changed my mind". He noted that "about 2/3 of our work" used rods from Hoover and Strong, although not from the same batch of metal.

The Wall Street article continues "The chemist also noted that it is highly unlikely that positively charged tritium atoms could escape from the negatively charged palladium. Therefore even if the palladium was contaminated, the tritium could not leak out into the surrounding heavy water, he argued". This statement may surprise scientists.

The Hoover and Strong Vice President, Dan Pharr said he was unfamiliar with Cold Fusion research - he worked for the jewelry trade and was not concerned about low level contamination.

People who are experts in palladium and reactors are not surprised as palladium is often used as a catalyst to combine gases in nuclear reactors. Appreciable tritium comes from CANDU reactors which use heavy water, less tritium will come from pressurized water reactors and almost none from boiling water reactors. The palladium is often recycled later. Hence it is not surprising that different samples of Palladium could contain very different amounts of tritium.

One of Dr. Wolf's cells, D6, did give large amounts of tritium. This was in late September and was after three months with no positive results, then just before a visit by EPRI funding agency, both cell D6 and number 4 of Bockris's lab, gave large amounts of tritium. The cell D6 was in front of Wolf's gamma ray counter which did not indicate any gammas - this could indicate that no nuclear process had taken place.

The Electrical Power Research Institution, EPRI, has given money for fuel cell work at Texas A&M for some years and increased their grant when Cold Fusion was announced. In the autumn of 1989, Texas A&M University asked for $1.4 million which was an order of magnitude more - it was for Drs. Bockris, Wolf and Appleby labs.

Dr. Wolf "said evidence that many of the experiments have produced low levels of neutrons is still 'pretty solid'".

David Worledge of EPRI which is funding many Cold Fusion groups including Drs. Wolf and Bockris, "explained that attempts to produce tritium in 'cold fusion' experiments didn't hold much further promise in trying to resolve the controversy. The question was more likely to be resolved by new experiments to detect neutrons and to explain the production of excess heat".

Thus while a short time ago tritium was considered the strongest evidence, now Believers are shifting to other experiments. This is in agreement with one of the characteristics of Pathological Science where the belief stays the same but the supporting evidence varies.

2. POSSIBLE FRAUD IN DR BOCKRIS'S LAB

2.1 Account

Science, the official journal of the American Association for the Advancement of Science, has published a long article by Gary Taubes about possible fraud in the tritium claims coming from the lab of Dr. Bockris. Because of the seriousness of writing about fraud, the article is very carefully written (it was two months in preparation). It is important to be clear about what it does NOT say;

1. It is does not say there was definitely fraud

2. It does not accuse any specific person of fraud.

However it does discuss the balance between academic freedom and the need to guarantee the integrity of Research. It does say that despite many warnings, both oral and by memos, Dr. Bockris and the Texas A&M authorities did not respond adequately to the problem. Also while no legal statement has been made, every person can make their own judgement of what is a reasonable explanation of the happenings described below.

The amounts of tritium production obtained in Dr. Bockris's lab are so enormous and so far from the close-to-background values obtained in other labs that they are frequently quoted by Believers and were instrumental in getting the $5 million released for setting up the National Cold Fusion Institute. At the First Annual Conference on Cold Fusion there were 15 groups reporting positive tritium production and as Chemical and Engineering News wrote, Believers "point to the observed emissions of tritium as the unassailable signature of a nuclear reaction".

On 22 April 1989, Nigel Packham of Bockris's group started looking for tritium in the electrolyte solution in the cell. He gave the 3 cells to the Cyclotron Institute who told him that there a trillion of atoms of tritium per millilitre "When I heard this number, my jaw dropped" said Packham. Similar huge quantities of tritium occured from time to time, and people noted that this "miracle" tended to coincide with important occassions such as a visit of a funding agency. One research student who left said that there were just "Too many goddamm 'miracles' in this laboratory" for him.

It can be noticed that the tritium counting rate tended to have a sharp spike and then a long fall-off which corresponded to the radioactive decay of the tritium and dilution of the electrolyte (as gases boiled off and the liquid level was restored) whereas if fusion was occuring for a few days, as excess heat observers claimed, then the counting rate would rise steadily for these days and then when the fusion stopped, the rate would slowly decrease. So the sharp jump could be interpreted as an unusually sharp burst of fusion or it could be interpreted as someone having spiked the electrolyte sample with tritium. Dr Appleby who was observing excess heat for long periods of time, was surprised by the tritium results and asked Dr. Bockris "Look, concerning this tritium - are you sure that someone hasn't been spiking your cells?"

In June 1989 when the DOE panel visited Texas A&M, Jacob Bigelstein, who is an expert on tritium, was particularly sceptical when he found that tritium was being claimed but no neutrons though by charge symmetry and experiments, 1 E5 to 1 E9 neutrons per second (a lethal dose) should have been observed. However Packham showed results for cell A7 where Bockris had wanted to catch a cell in the act of producing tritium. The curve showed zero counts at noon, a very slight increase at 2 pm, 500 000 disintegrations per minute per ml (dpmml-1) at 6 pm and 760 000 dpmml-1 at midnight. Packham had drawn a smooth curve through the points indicating a smooth rise in the tritium rate but Bigelstein said "Well, your data do not uniquely define that curve, I could equally well draw the following kind of graph through your data - go flat across at zero until a point around six hours, go straight up with a step function and go flat across again" Kevin Wolf said "Jake are you implying that someone spiked that sample?" Bigelstein replied "Kevin, you said that. I would never say such a thing".

Normally cells are followed for weeks or months, but it seems cell A7 was only followed for 12 hours. Also since this experiment was so important, it is astonishing it appears not to have been repeated in almost a year. Another surprising feature of this critical experiment was that tritium was observed within six hours whereas Dr. Bockris and other Believers often claim it takes weeks or months.

Bigelstein asked if there was any tritium in the lab and Packham said there was a bottle of tritiated water, five millicuries worth.

In a 18 December memo to John Fackler, Dean of the College of Science at A&M, Bockris wrote "This possibility (that the tritium was put there by someone), has been taken seriously by us from the beginning.... we have monitored a certain
flask containing tritiated water purposely left in its original position. Not only did we note the original level of the water in the flask but also we measured its tritium content. It has remained unchanged..." However this is in disagreement with a memo dated 4 September from Packham which says that there are many bottles uncontolled in the lab.

It was claimed the cell was carefully guarded, e.g. in November Packham said the cells "were under guard for that time 24 hours a day, 7 days a week. There was one cell (A7)... that shows the build up of tritium as a function of time,

where four people were standing there the whole 12 hours in front of the cell when the samples were taken". However Ramesh Kainthla, an Indian post doc who was the senior member of the team, who took the samples at 6 pm and midnight, said " If you think people were watching the cells all the time that is not true. Watching the cell meant a person in the lab, and once in a while (that person) came in and checked that the current was passing." "If you want to do some mischief, you do not need a couple of hours. You can do it in a very, very short period of time."

In July, Charles Martin who was working on Cold Fusion but not getting positive results, suggested to Bockris at a meeting, that he would run Bockris's cells in his, Martin's lab, and restrict access. However Bockris did not take this offer. However he did take Packham off the job of sampling the tritium "I tried to take Packham off" Bockris says, "because by that time all these stories were floating around. Nigel spikes the tritium. Everyone thinks Nigel spikes the tritium."

Dr Martin copied Bockris's cells and procedure carefully and he controlled access. For the case of two cells with Palladium donated by Bockris, he even ran them at home in his second bedroom. Finally in January, Dr. Martin wrote the final results to Dean Fackler - that none of the 83 cells had given significant signs of tritium.

When Fackler asked Bockris why Martin could not replicate his results, he replied on February 2nd "My tentative judgement of today is that a new field of chemistry has been formed. As for "why cannot Dr. Martin succeed?... we cannot succeed either for long periods of time (e.g. 6 to 8 weeks). The important thing is when we do succeed which may be 10 weeks after we switched on the electrolysis." Yet miracle cell A7 gave tritium in only six hours.

In the group's first paper is written "interference with the experiments is considered improbable because of positive results from the Cyclotron Institute to which entrance is prohibited except by the usual personnal at the Institute." Indeed when Kevin showed me his work, I had to sign in and was given a monitor. Also Kevin had to unlock the door of his neutron counting room. However it turns out there is no guard nights or weekends and Dr. Youngblood, the Director of the Institute, told Dean Fackler that "at least 35 faculty and lab personnal had keys that would open that door".

The above is consistent with No. 5 of Irving Langmuir's six characteristics of Pathological Science: "Criticisms are met by ad hoc excuses thought up on the spur of the moment." However on controlling and thinking, these excuses do not stand up. This has often happened to me with Cold Fusion.

These are very suspicious hints of fraud and the Science article contains much more, but is not absolute proof, though it would suggest that precautions should be taken. However stronger evidence on the possibility of fraud came from Kevin Wolf when he tested the electrolyte from a fusion cell run in Dr. Bockris's lab and which had shown a high level of tritium. It had been sealed in a container since the previous year. He found it contained large amounts of light water. Now if there were fusion of the D2O there should be no H2O produced, but if the sample had been spiked with tritiated water from a bottle one would expect some H2O. On hearing this I contacted Texas A&M and was told there was an explanation - during normal operation there was some contact with the air and H2O could have got in(or as John Fackler put it "there's a concern about that... it is possible that the normal water is just condensation from the moist Texas atmosphere). This sounded to me like Characteristic No. 5 again and I suggested that quantative tests be done comparing the amount of H2O in the sample with tritium with that in other cells with no tritium. However this had been done - Nigel Packham and others had tested 8 cells, two of them sealed, and found 30 to 90% H2O, an enormous amount while Kevin Wolf checked 50 cells in his own lab and "found no more than 1% - usually much less in 48 of them." This might seem strong evidence in favour of spiking but Packham has told Science that he and Bockris are not ready to abandon their results.

Kevin Wolf said that "The proper conclusion is that things (in the Bockris lab) were so uncontrolled and so sloppy (that) those studies don't mean anything."

According to AP, John Fackler said last week that "he had no reason to believe that fraud had occurred and that there were no plans to investigate the cold fusion experiments." "'I have no concrete evidence of anything other than fairly sloppy chemistry.'"

At the NCFI, the Director Dr. Will said that their source of palladium was not from Hoover and Strong. He said the amounts of tritium they had seen, about 1/2 to 3 times background, were so low "we have not made any big point of them." According to the SL Tribune he said the Institute has begun

"double-blind" tests to search for tritium. He said 24 labs world-wide had seen tritium but only BARC had reported amounts comparable with the Bockris results but as complete scientific papers from BARC have not been forthcoming, "Nobody is really in a position to scrutinize these results."

2.2 POSSIBLE CONCLUSIONS

In view of this information there would seem to be two reasonable interpretations;

A) there was fraud

B) there was very sloppy science

Either way, the claims of Dr. Bockris's group should be excluded from compilations of results as unsafe.

Experience has shown it is very difficult to prove fraud in a court of law. However scientists who are accustommed to studying lots of data and drawing their own conclusions, can decide for themselves whether the probability of fraud is 50%, 90% or 99% or whatever.

3. OTHER TRITIUM CLAIMS

At the end of the First Annual Cold Fusion Conference, a Los Alamos document was issued which listed 15 labs reporting the observation of tritium (my notes give a lower number, but it is not very important). As far as my notes go, only two of these reported enormous production of tritium. One was Dr. Bockris's group which is discussed above, and the other was the Bhabha Atomic Research Centre, BARC in India. The latter's results tend to show a sharp rise and then a descent similar to results of Bockris. Thus the possibility of spiking should not be excluded - it might be wise if they were to repeat Dr. Martin's technique of taking one set of samples of cells and isolating them (though not necessarily taking them to their bedroom!)

With the BARC work there are two differences;

1. The tritium results were obtained by several different divisions of BARC and it is said that these divisions are independent.
2. "A unique feature .... is that the first bursts of neutrons and tritium occurred (in 8 out of 11 cells) on the very first day of commencement of electrolysis, when hardly a few Amp-hrs of charged had been passed."

The first of these could be considered strong evidence against spiking while the second could raise doubts.

Several groups at BARC also measured neutrons. They claim that the ratio of tritium to neutron production is 1 E6 to 1 E9 (though there are also values of 1 E3 and 1 E4) and the Bockris/Wolf groups also claim ratios of about this. At the First Annual Cold Fusion Conference this was tuned to 1 E8 and this value was repeated as a criterion that satisfactory theories should meet - and some did! However there are a very large number of experiments which have proved that this ratio is very close to one and hence in agreement with charge symmetry and not 100 000 000 as Cold Fusion Believers suggest.

In a voluminous "Review of the Investigations of the Fleischmann - Pons Phenomena" by Bockris, Lin and Packham, graphs are shown of the variation with time of the tritium counts for 5 labs. Four of them are consistent with the sudden occurrence of tritium and then decay while the fifth is different in that there appears to be frequent increases and decreases in the counting rate so that it could not be due to a single afflux of tritium. However there are two worrying features about this experiment;

1. The counting rates are very low, 100 to 400 dpmml-1. Now the DOE panel report says that D2O normally contains some tritium giving counting rates of 120 to 180 dpmml-1. Also due to different characteristics of d and t nuclei, there is electrolytic enrichment causing the amount of tritium to increase, so that special care is needed to consider values of less than 1000 dpmml-1 as anything other than electrolytic enrichment
2. There are reports that there is a nearby building that occasionally vents off tritium and also a nearby accelerator which can greatly increase the background. There are no detailed reports of adequate precautions being taken by the authors to avoid such local contamination.

4. UNIVERSITY OF UTAH STARTS TO CLEANSE ITSELF

4.1 Legal Letters

The letters that Mr. Triggs the personal lawyer of Drs. Fleischmann and Pons (probably mainly Pons as he is an old friend of Pons and lives in North Carolina) sent Mike Salamon and his co-workers asking them to retract a published paper and enjoining them to silence, have caused great offense. The American Physical Society will discuss this at its next

executive meeting. Also members are offering to help with a legal defense fund. However that should not be necessary as Joseph Taylor, who is a law professor and vice-president for academic affairs has said the university will defend Salamon et al. if he is sued "in the line of duty." Note that solves the retraction problem, but if Mike makes a statement outside the line of duty, then he could still be sued and the University might not be able to defend him! However a little sanity has returned (would like to think that my series of protests to Martin Fleischmann helped since in the past he has often corrected excesses). Nature writes that on June 5, Mike "received a letter from Triggs apologizing for 'any concerns or misconceptions' his first letter may have caused and assuring him that there is no intent 'to limit in any way the lawful excercise of your academic freedom'. The letter adds that Pons and Fleischmann now 'intend to settle (the) dispute in the court of science through publication". Better, but note that the people who asked Triggs to write the first letter have not themselves commented, and also there is still an implied threat to Mike that he must excercise his freedom "lawfully" - am sure he will, but the law of Tort is still a minefield for scientists who believe that if something is true, you can say it.

From what I have heard, Prof Taylor is a wise person - immediately after the 23 March 1989 press conference he told Chase Peterson in strong terms that it was a major mistake.

That the University paid Triggs $68 000 for patent work although he is not a patent lawyer, has raised some questions.

4.2 Resignation of President Peterson and NCFI Audit

After the College of Science Dean, Hugo Rossi, and 22 professors protested about misuse of $0.5 m of funds being offered to the National Cold Fusion Institute, NCFI, the University president, Chase Peterson agreed to a scientific audit to be conducted by the faculty and said its members would be appointed in the next few days. Also there would be a financial audit. He said he would not resign, "I've considered it hundreds of times before in the past seven years, and I will consider it again in the future".

On Monday 4 June, the Institutional Council, the U's governing board met and the chair, James Jardine supported Peterson, though others were less strong. But in the afternoon the Academic Senate met and passed a resolution which read in part "The academic senate respectfully requests that the Institutional Council and the Board of Regents examine the question of whether continuation in office of the current president is in the best interest of the University of Utah and the community which it serves". The resolution was proposed by a History professor and seconded by a professor of Chemical Engineering. A professor of Human Genetics said "the university cannot continue to lurch from crisis to crisis" and an English professor said "I've resisted this moment for a number of years.... but it seems to have come to this." It appears that there were many things that people were unhappy about and Cold Fusion was the straw that broke the camel's back.

Dr. Pons returned from Europe and gave an off-camera interview where he said he would co-operate with the audit and in fact he was ready to turn over all of his raw data to a review team, with one reservation - he said they had written a 66 page definitive paper that he hopes will be published next month and he wants to hold on to the data until after the paper comes out. This reservation at first may sound reasonable but is ridiculous as at the First Annual Cold Fusion conference at the end of March, Pons gave and distributed a paper of 25 pages plus figures in addition, which he said contains the essentials - so this looks like a typical delaying tactic. At present even Hawkins does not have access to the raw data even though he did most of the work. It is to be hoped that the Senate audit committee will insist on obtaining the raw data immediately to avoid any chance of them being accidentally lost (there are rumours of a critical tape containing raw data on the gamma peak from neutrons being accidentally wiped clean).

However the Salt Lake Tribune wrote that on 11 June, President Peterson announced he would retire in 1991. '"It is my intention to dedicate this remaining year to accomplishing my remaining goals and to position this university for new leadership following the 1990-91 year. At this time I will move on to other opportunities". Despite prior discontent, faculty members pledged their support to the president.'

The NCFI Director, Fritz Will has said he will refuse the controversial $0.5 million. Funding is becomming a still bigger problem for the Institute as their hopes of getting $160 000 from EPRI decreased when David Worledge of EPRI said "We should not proceed with the contract negotations until the

dust settles."

The F/EAC (the NCFI supervisory committee) is in the process of choosing members for the two external committees for science and management/financial reviews of NCFI. Some people are unhappy about this since the history of the F/EAC has not been brilliant. Are they the best organisation to choose people which will investigate also their role? And will the review committees contain people who are well-informed sceptics of Cold Fusion? Who decided that the F/EAC was the best organisation to choose a review committee since the University was supposed to do it?

The SL Tribune says that Dr. Bockris was at one time a candidate for the position of Director of the NCFI.

People who turned down offer of jobs from the University of Utah have received a questionnaire asking them their opinion of the State, of U. of U. etc. Cold Fusion and NCFI have been mentioned in the answers.

5. COLD FUSION EXPERIMENT AT THE KAMIOKANDE DETECTOR

The Japanese experiment Kamiokande has probably the best detector of neutrinos from the sun and from any nearby supernova as its results for SN 1987A showed. They have decided to place a Cold Fusion cell in the heart of their detector towards the end of this year. The point is that while well-informed Japanese scientists tend not to believe in Cold Fusion, there is no major Japanese experiment that has been done on it. Also the public are not well informed. Thus in the magazine Kagaku (which is the Japanese equivalent of the Scientific American) I have just found out that my notes reviewing ALL Cold Fusion experiments were preceded by an article where Dr Ikegami of the Japanese National Fusion Institute mentioned only the results presented at the First Annual Cold Fusion Conference and which were only positive and where he concluded that there should be something in it.

The cold fusion experiment would be done this year and I am assured that it will not significantly interfere with their work on Solar neutrinos which is of great interest as was shown by the controversy at the recent Neutrino '90 conference where it was not clear whether the theoretical flux of solar neutrinos was different from the experimental values. The cell is so small compared with the large volume of the Kamiokonde detector, that the effect of its presence is negligable.

6. FUTURE CONFERENCES ON COLD FUSION AND ANOMALOUS EFFECTS

Despite recent problems, conferences continue to have sessions on Cold Fusion and there is one conference devoted to it. This is at Brigham Young University. It is markedly different from certain other conferences where only positive results were presented. It is called "Anomalous Nuclear Effects in Deuterium/Solid Systems". The requirements for papers are clearly stated, e.g.

"3. Anomalous Tritium Production in Deuterium/Solid Systems
Papers are requested that support or refute the anomalous production of tritium in such systems. Only those experimental results that include comparitive blank runs and documentation of initial tritium content should be presented."

This is very different from some recent conferences and one detects the hand of Steve Jones.

Abstracts should be sent to Steve before 15 September. The Technical Secretary of this International Workshop is Nate Hoffman at Rockwell International Corp., PO Box 1449, Canoga Park, Ca 91304. Fax (818) 700 5118.

7. WHEN DID COLD FUSION BEGIN?

Recently people have been trying to find out when Cold Fusion started. Steve Jones has a document witnessed by a notary, which gives the date for BYU as 1986. But when did Pons and Fleischmann start? For their patents to be sellable it is the date of the first experiment which counts, not the first to give a press conference. They have repeatedly emphasised that they had been working on Cold Fusion for five years before March 1989, but have not seen or heard of any firm evidence that justifies this claim. It appears that their first successful "experiment" was the melt-down of their palladium block. According to Time magazine (8 May 1989) this was in 1985 but there are other indications that this only occured in about the summer of 1988. And their first conventional experiments may have started in April/May 1988 and they began to get results in December 1988, i.e long after Jones et al. These dates are not well established but when Dr. Pons makes his lab notebooks available to the enquiry as he has promised,

it will become clear.
It is not sure whether the "melt-down" of the palladium block could be considered an experiment as it occured at night when no one was present and there were inadequate measuring and recording devices.

8. PROBLEMS OF DEALING WITH FRAUD

Scientists are educated to study and believe experimental results. And when young they do. Their culture makes them very trusting and there results an exceptionally good working atmosphere. However magicians such as Randi say that scientists, especially physicists, are the easiest people to deceive. This is because they virtualy never encounter fraud in their work and rarely hear lies.

The consequence is that when there are good grounds for suspecting fraud, scientists generally do not know how to deal with it.

Let us distinguish two cases;

1) If there is no suggestion of fraud, then for the sake of academic freedom and for good working relations with your colleagues, then I feel very strongly that one should not start or have a special investigative office.
2) If there are serious doubts and if they have been expressed widely, then the the priorities are different. The reputation of the supervisory institution is at risk. It and the people implicated need to have a clear opportunity to defend themselves. In other words an enquiry should be set up as soon as possible.

Institutions often defend themselves by internal enquiries which are whitewash jobs. From the historical point of view, this often succeeds but it leaves a bad taste and good people tend to leave the institution. Dick Feynman's wonderful account of the Challenger enquiry in his last book, is an example of a whitewash that mainly succeeded apart from Dick's actions.

Supervisors sometimes feel that inviting in the person for him to explain is an adequate enquiry but there is the fundamental problem that it is very difficult to say to a colleague to his face that there are doubts. To have a real enquiry it is necessary to have outside people who are experts in the subject. I was at the 23 March 1990 meeting of the National Cold Fusion Institute supervisory board where the dubious results and the even more dubious financial statement were approved and the correct questions were sometimes asked but mildly and and any answer was accepted with little thought as to whether it was correct or adequate - the questioning seemed a formality.

So the fact that someone was questioned and gave answers does not mean very much. It is necessary to have a record of the questions and answers which are agreed by all, for often the explanations change with time or turn out to be untenable when checked. And a follow up is needed to check, often experimentally, if the explanations are correct. Note that what is suggested here does not in any way restrict academic freedom.

In conclusion, fraud is very, very rare, but if a number of people seriously consider it the most likely explanation, then it is best to have an independent enquiry quickly.

Douglas R. O. Morrison.

PS. LETTER OF DR STORMS TO SCIENCE

Have just received a copy of the letter that Dr. Storms of Los Alamos has sent to the Science magazine on June 25. It contains some results that he said he sent to Gary Taubes on 9 April.

In it he describes adding some tritium to one of his cells (it is a semi-closed cell with a catalyst) and he compares the variation with time of the tritium counting rate and the ratio of counts from the electrolyte and the gas, with what was observed with Dr. Bockris's cells which gave tritium counts. He concludes the two factors are different and hence the suspicion of the fraudulent addition of tritium is irresponsible.

As Dr. Storms letter does not seem to address the basic problem, some comments will be made;

1. Firstly it is important to note what was NOT considered in his letter;
   a) That light water was observed in those of Bockris's cells that gave tritium
   b) That the amount of light water was 30 to 90% in the cells that gave large amounts of tritium but only 1% or less in Kevin Wolf's cells that gave little or no tritium.

2. It was an excellent initiative of Dr. Storms to try the experiment of adding

tritium but two main features should have been taken into consideration;

c) the basic question of light water - he should have added a tritium -light water mix which could reproduce the final mixture of the sample with 30 to 90% light water and a tritium counting rate of about 1 000 000 dpmml-1.

d) One should compare like with like. Apart from the fact one cell was closed and the other open, there are two major differences between the the Storms and Bockris experiments;

i) The tritium counting rate in the Storms experiment increases over some 20 days and is thus inconsistent with a single spike. In the published results of Bockris et al., the rate jumps suddenly up and the increase seems to occur in six hours or less.

ii) The counting rates in the two experiments differ enormously, so different that the two experiments cannot be safely compared.
In Storms's experiment the counting rate is very low, in the hundreds of dpmml-1.
In the Bockris experiments the counting rate is about a million dpmml-1.
The rate in the Storms experiment is so low that the DOE panel warned that with the increase in tritium due to separation by electrolysis, one should treat counting rates of less than 1000 dpmml-1 with care.

It is perhaps of interest to compare the power estimated from the experiments of Storms, Bockris and Fleischmann and Pons.

Assuming Storms increases his counting rate by 100 dpmml-1 in 10 days, then the average power is about 1 E-9 Watts or a nanoWatt (note 100 dpmml-1 is about the normal content of tritium in D2O - it varies from about 75 to 200 dpmml-1).

Assuming Bockris achieves a rate of 7 E5 dpmml-1 in 10 hours, then the average power would be a few E-4 Watts or about less than a milliWatt from all sources.

Fleischmann and Pons claim to have measured excess heat of about 10 Watts which would have given a tritium rate of 1 E11 dpmml-1. In their first paper they indicated a tritium rate corresponding to less than 1 E-7 Watts or a tenth of a microWatt.

If someone should wish to test experimentally the effect of spiking as a possibility, then it would be best done by adding a tritium/H2O mixture identical to that in the bottle in Dr. Bockris's lab (the mix is known since Dr Bockris said he had measured the radioactivity and the level in the bottle) Then an amount should be added which gave 30 to 90% H2O.

Scientists do not like the idea or suspicion of fraud. It would be nice if it were to just to go away. But with a vigourous investigative journalist who is finishing has a commercial aspect, this is unlikely. A rapid external enquiry might be the best way to settle this very, very rare occurrence.

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Neutron Physics Division

11F

June 21, 1990

Dear Dr. Garwin,

This has reference to your fax dated June 11 addressed to Dr. P.K.Iyengar. We have also heard recently about the experiments of Wolf wherein he has found tritium in unelectrolised Pd. In fact the possibility of tritium being present in the Pd has been discussed at BARC right from the beginning. As such many of the electrolysis experiments used electrodes which were thoroughly degassed under vacuum at temperatures of 900°C for several hours. One group even produced the Pd pellet by starting from specpure palladium powder. In view of the fresh evidence for such suspicions Dr.T.S.Murthy and his colleague at BARC are writing up in detail the preparation and pretreatment of some of their electrodes.

Meanwhile I am sending you by post a copy of the review paper entitled "Overview of BARC studies in Cold Fusion" which we had presented at Salt Lake city recently. This is an up to date review of all the work done in BARC as of April 1990.

Let me conclude by saying that as of today I ~~am~~ (and others) in BARC have no doubts about the occurence of nuclear reactions in Pd and Ti. In fact I am quiet excited by the microcurie levels of Tritium we are finding in the centrol Ti anode of our plasma focus experiments. This is briefly reported in the overview paper. But a more detailed version is under preparation for publication.

With best regards,

Yours sincerely,

M Srinivasan

(M. Srinivasan)

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OVERVIEW OF BARC STUDIES IN COLD FUSION

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Invited Paper
Presented at

THE FIRST ANNUAL CONFERENCE ON COLD FUSION

Held at
SALT LAKE CITY
UTAH, USA
on
MARCH 28th – 31st, 1990

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Chairman
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OVERVIEW OF BARC STUDIES IN COLD FUSION

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ABSTRACT

A wide variety of experiments have been carried out by twelve independent teams employing both electrolytic and gas phase loading of deuterium in Pd and Ti metals to investigate the phenomenon of cold fusion first reported by Fleischmann and Pons in March 1989. The experiments were primarily devoted to the study of the emission of nuclear particles such as neutrons and tritium, with a view to verify the "nuclear origin" of cold fusion. In 22 different electrolytic experiments whose cathode surface areas ranged from 0.1 to 300 cm^2, large bursts of neutrons and/or tritium were measured. Some of these gave clear evidence that these two nuclear particles were being generated simultaneously. The neutron–to–tritium yield ratios in majority of these experiments was in the range of 10^{-6} to 10^{-9}. The specific neutron and tritium yields expressed per cm^2 of cathode surface area also fitted into a systematic pattern. A unique feature of the BARC electrolysis results is that the first bursts of neutrons and tritium occurred (in 8 out of 11 cells) on the very first day of commencement of electrolysis, when hardly a few amp–hrs of charge had been passed.

In gas phase studies copious neutron emission was observed in a Frascati type absorption/desorption mode experiment with Ti shavings. Presence of tritium in D_2 gas loaded Pd and Ti samples was established through the technique of autoradiography as well as Ti K X–ray counting. In the case of Ti, it was noted that RF heating of samples, in leau of resistance furnace heating, somehow promotes tritium formation. Most recently it was found that $\approx 10^{16}$ atoms of tritium had been "produced" on the top end surface of the central Ti electrode of a deuterium filled Plasma Focus device after it was subjected to $\approx$ 80 charge/discharges shots. All in all the BARC studies have unambiguously confirmed the anomalous production of neutrons and tritium in deuterium loaded Pd and Ti lattices.

1. INTRODUCTION

The announcement in March 1989 by Fleischmann and Pons /1/ of the occurrence of (d–d) fusion reactions (or possibly some other unknown nuclear processes) in Pd metal cathodes electrolytically loaded with deuterium , followed by reports of the observation of "2.45 Mev fusion neutrons" independently by Jones et al during the electrolysis of D_2O, resulted in a frenzy of activity the world over to reproduce these measurements. At Trombay several groups having expertise in various areas such as hydriding of metals, electrochemistry, isotope exchange processes in the upgrading of heavy water, fusion plasma experiments and neutron and tritium measurements devised and set up a variety of electrolytic cells during the early days of April 1989. In a centre such as BARC which has nurtured the development of the heavy water moderated line of reactors in India for over three decades, equipment and expertise for the measurement of neutrons and tritium was readily available. In the initial experiments the emphasis was naturally on the detection of nuclear particles rather than "excess heat" which required intricate calorimetry. The first positive evidence for the emission of neutrons and tritium was obtained on 21st April 1989 and since then several different cells have confirmed these results.

Meanwhile reports from Frascati of the detection of neutrons from pressurized D_2 gas loaded Ti shavings /2/ opened up a second channel of cold fusion investigations. Neutrons / or tritium have since been measured in a variety of D_2 gas loaded Ti and Pd targets at Trombay A brief summary of the early BARC work /3/ was presented at Karlsruhe in July 1989. Report BARC–1500 issued in December 1989 is a compendium of twenty papers, documenting in an informal style the status of ongoing work, and

covers "BARC Studies in Cold Fusion" over the period April to September 1989 /4/. The experimental papers of this compilation are also being published in Fusion Technology /5/. The present paper is an overview of all the experimental work done at Trombay during the first year of the 'cold fusion era' including new results obtained since the publication of BARC–1500 and summarizes the efforts of about a dozen independent groups comprising over 60 scientists and engineers drawn from different divisions of BARC.

PART A: ELECTROLYTIC STUDIES

2. EXPERIMENTS WITH HIGH CURRENT NaOD ELECTROLYZERS (HWD/N_tPD/DD)

2.1 NEUTRON MONITORING

Three different neutron detectors were available for monitoring the neutron yield in these experiments. The first was a bank of three BF_3 counters (each of 25 mm dia x 450 mm length) sensitive mainly to slow neutrons, embedded in a paraffin moderator block. The second was a similar bank of three paraffin encased thermal neutron detectors except that they were of the He^3 type. The third neutron detector was an 80 mm dia x 80 mm high proton recoil type plastic scintillator (NE 102A) sensitive both to fast neutrons as well as high energy gammas. During the electrolysis experiments usually one of the thermal neutron banks and the fast neutron detector were mounted close to the cell while the other thermal neutron bank was located about 1.5 m away to serve as background monitor. The neutron detection efficiency was determined with the help of a calibrated Pu–Be neutron source placed at the cell location and was typically in the region of 0.05% to ~1.5%. In later experiments a personal computer became available to display on line the count rate variations.

2.2 MILTON ROY COMMERCIAL ELECTROLYZER/5/

A diffusion type water electrolyzer using Pd–Ag alloy tubes as cathodes designed to generate ultra pure (oxygen free) hydrogen gas had been procured from the Milton Roy company of Ireland, sometime in 1988 for the purpose of generating D_2 gas for use in Plasma Focus experiments. Thus it so happened that when news of the cold fusion phenomenon reached Trombay in March 1989, this cold fusion cell was all set to be switched on with D_2O as electrolyte. 5M NaOD in D_2O was selected as the electrolyte based on the recommendation of the suppliers of the Milton Roy cell. A schematic view of this cell is shown in Fig.1. The outer nickel body along with a central Ni pipe serve as coaxial anodes. The cathode comprises of 16 numbers of specially activated Pd–Ag alloy membrane tubes having a total surface area of 300 cm^2.. These tubes are sealed at the top and open at the bottom into a plenum through which the D_2 (or H_2) gas is drawn.

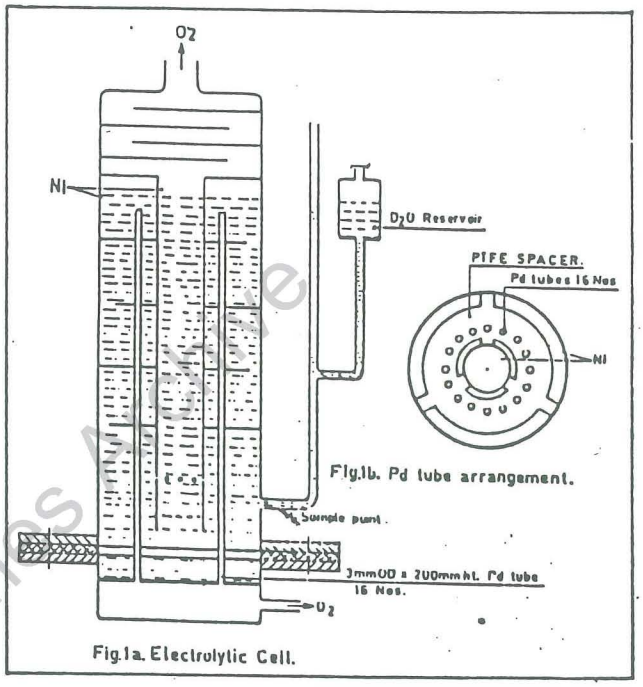

Fig.1 Schematic View of Milton–Roy Commercial Electrolyzer

D_2O of >99.75% isotopic purity, containing ~0.075 mCi/ml (or ≃2.8 Bq/ml) of tritium and moisture free Na were employed for preparing the NaOD electrolyte. The electrolyzer can be operated upto a current level of 100 amps corresponding to a current density of ~330 mA/cm^2, although for continuous operations only 60 amps is recommended in order to avoid overheating.

Run No.1 (21st April 1989)

The cell was initially operated for about 48 hours with 20% NaOH in ordinary water. It was later flushed with D_2O and filled with 20% NaOD solution in D_2O prior to commencement of electrolysis on 21st April 1989. Following brief operation at 30 amps, the current was slowly raised to 60 amps. After about 3 hours at this current level both the neutron detectors viewing the cell started showing counts well above background values. At this time the current was raised further and this resulted in a number of distinct neutron peaks appearing in both neutron detector channels (See Fig.2). The experiment was

terminated when the cell current increased on its own to over 120 amps at the time of the last peak, resulting in the power supply getting damaged. The total number of neutrons generated during the four hour duration of this run is estimated to have been ~4 x 10^7.

the large burst of 21st April.

Electrolysis commenced on 12th June at a current level of ~60 amps. The BF_3 neutron detector bank monitored the cell while the He^3

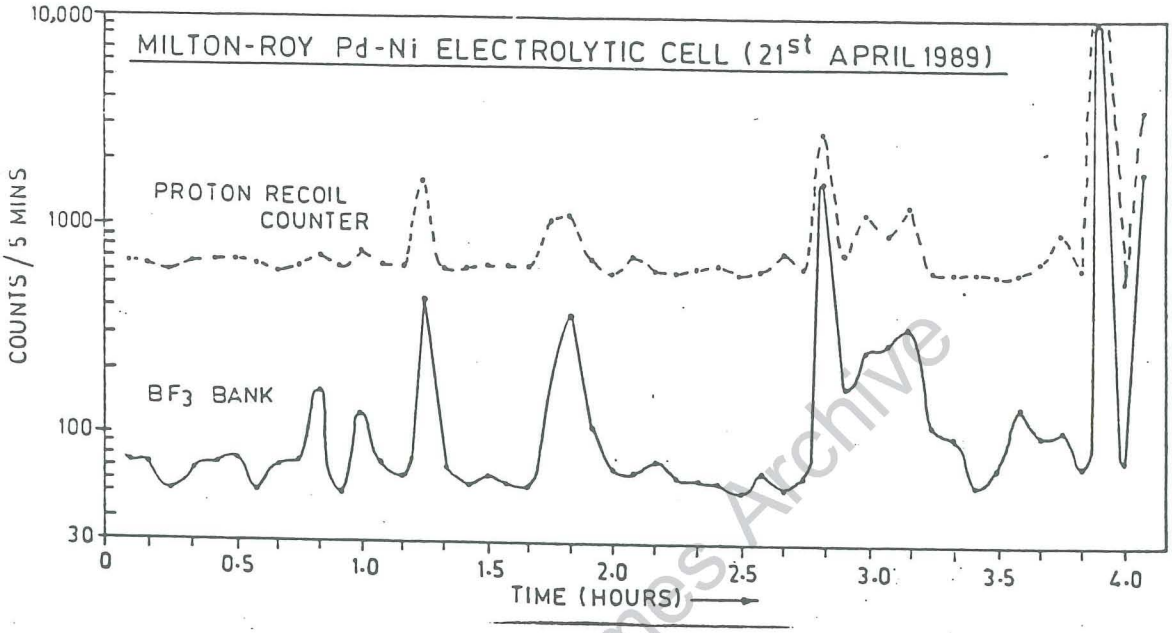

Fig.2 Neutron Counts Variation During Run No. 1 of Milton Roy Cell (21st April 1989)

A sample of the electrolyte drawn after the run, indicated a tritium content of ~1.5μCi/ml (55.5 KBq/ml), an increase by a factor of ~20,000 in comparison to the tritium content of the initial stock heavy water. Taking into account the total volume of the electrolyte (250 ml) as well as the amount of make up D_2O added, it is estimated that an excess of ~8 x 10^{15} atoms of tritium were generated in this run. The corresponding neutron–to–tritium yield ratio works out to be 0.5 x 10^{-8}. This was the first indication to us that the neutron–to–tritium yield ratio in cold fusion experiments is anomalously low.

Run No.2 (12th to 16th June 1989)

A second series of electrolysis runs was carried out with this cell in June 1989 after it was drained and thoroughly flushed with D_2O several times. Prior to this a drain tap with a valve had been welded to the bottom of the cell to enable periodic withdrawal of electrolyte samples. Fresh electrolyte solution prepared with stock D_2O was charged and left in the cell over a weekend. A sample of this electrolyte taken on the following Monday morning gave a high tritium level of ~0.32 nCi/ml (11.8Bq/ml), presumably due to leach out of tritium left over in the cathodes from bank served as background monitor. Except for a few small neutron bursts which were observed within about half an hour of commencement of electrolysis, no neutrons were recorded for the next couple of days, although electrolysis continued until 17.45 hrs of Wednesday 14th June, when the cell was put off. A couple of hours later there was another small neutron burst lasting ~15 minutes. (These small bursts are shown plotted in Fig.1 of the companion paper from BARC /6/.) But on the evening of Friday 16th June, there was a large neutron burst (>10^6 neutrons) lasting for a couple of hours. (see Fig. 3)

The week long experiment was terminated at this point but the electrolyte was left in the cell and the D_2 gas plenum closed leaving the gas at an excess pressure of ~ 1kg/cm^2 above atmospheric pressure. Samples of electrolyte were drawn every day during the week of the experiment and sent for tritium analysis. The sample drawn on 23rd June indicated a high tritium level of 121 nCi/ml (4.5KBq/ml). After a lapse of about a month the tritium level in the electrolyte was found to have further increased to a value of ~460 nCi/ml (17KBq/ml), a four fold increase since the termination of the experiment. Fig.3 shows the variation of tritium concentration

during the entire course of Run No.2. It may be noted that after the large neutron burst the tritium level has shown a thousand fold jump suggesting that tritium is produced at the same time as the neutrons.

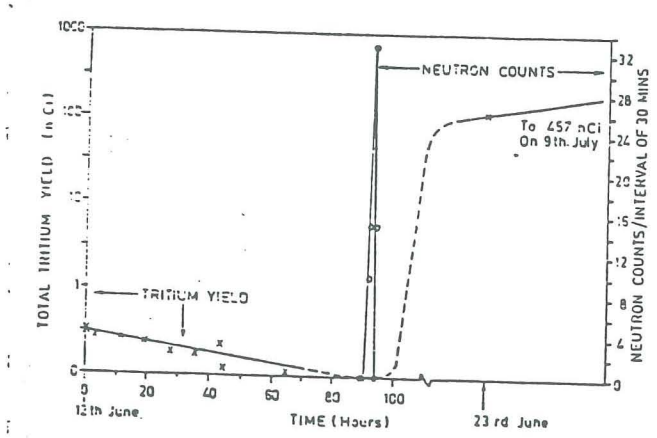

Fig.3 Neutron and Tritium Output During Run No.2 of Milton–Roy Cell

The integrated neutron yield during this experiment was ~0.9 x 10^7 neutrons while the total number of tritium atoms generated was 1.9 x 10^{15}. The corresponding neutron–to–tritium yield ratio is 0.5 x 10^{-8}. This is in remarkably good agreement with the results of the earlier Milton Roy run, although the absolute neutron and tritium yields are lower by a factor of ~5. Thus the Pd–Ag cathode appears to have partly lost its capability to support "cold fusion reactions" after the first run..

2.3 FIVE MODULE CELL WITH DISC ELECTRODES/5/

This modular five unit cell represents our early attempts at obtaining experience with an electrolyzer design which can be scaled up to higher capacities if required. Five cathode discs each of 78 cm^2 area and 1 cm^3 volume are fabricated out of Johnson & Matthey palladium(75%)–silver(25%)–alloy sheets (0.12 mm thickness); the anode plates are made of porous nickel. The individual modules of the cell are clamped together and connected in series. Fig.4 gives a schematic sectional view of the cell. The cell is capable of operating at currents of upto 80 A corresponding to current densities of ~1A/cm^2. Operating electrolyte temperatures close to 100 C are possible as the unit can withstand internal pressures of a few atmospheres.

Two neutron detectors, namely a BF_3 bank and a fast neutron recoil detector, were mounted close to the cell to monitor the neutron output. The system was filled with freshly prepared 20% NaOD in D_2O on 5th May 1989 and electrolysis commenced at a current of 60 to 65 amps (applied voltage was 12.5 V). When the cell had operated for about four hours a big burst of neutrons overlapping two consecutive counting intervals was recorded in both channels. Knowing the neutron detection efficiencies it is estimated that ~5 x 10^6 neutrons were generated during that burst. It was found that the tritium level had jumped by a factor of ~3500 from an initial value of 0.055 nCi/ml (2Bq/ml) to a final post burst value of 190.3 nCi/ml (7KBq/ml). Considering that the total inventory of electrolyte in the system was ~1 liter, this corresponds to an overall excess tritium production of ~190 nCi or 4 x 10^{15} atoms. It must however be emphasized that this does not include the tritium carried away by the electrolytic gas stream which was allowed to escape. Thus an upper bound to the neutron–to–tritium yield ratio in this experiment is 1.2 x 10^{-9}.

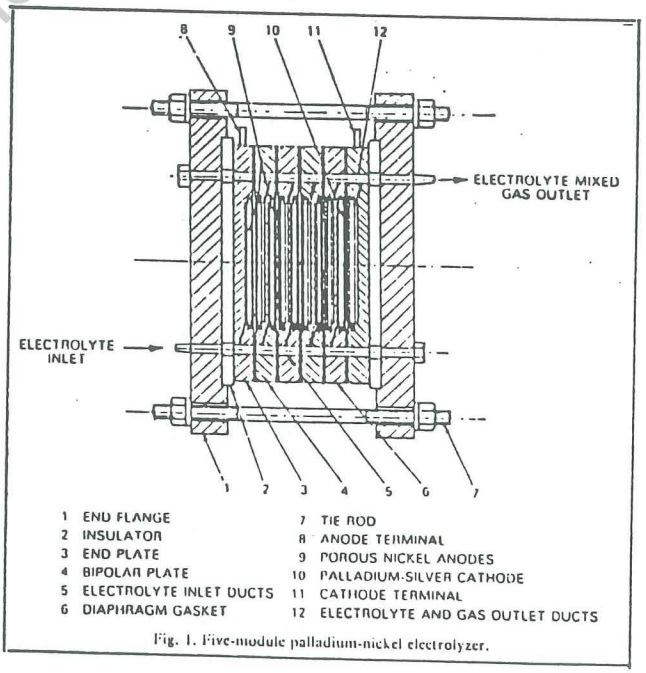

Fig.4 Schematic Sectional View of Five Module Cell With Disc Electrodes

2.4 EXPERIENCE WITH A Ti–SS CELL/5/

A Ti–SS cell was quickly fabricated in April 89 using readily available components in order to obtain some experience with use of Ti as cathode material. A 22 mm dia x 150 mm long rod (surface area = 104 cm^2) of Ti with a flange at the bottom served as cathode. An SS pipe of 40 mm ID served as anode leaving an annular inter

electrode gap of ~9 mm. PTFE gaskets at the bottom ensured coaxial alignment, as well as leak tightness. A vent at the top permitted free escape of electrolytic gases. A second cell of identical design was also fabricated for use as a control cell with H_2O.

5M NaOD in D_2O was used as electrolyte. The current density was ~400 mA/cm^2. The main problem with this cell was the continuous deposition of a dull black coating of iron on the cathode which impaired operation. The electrolytic solution also developed a pale greenish yellow colour. The electrode surface had therefore to be cleaned frequently and fresh electrolyte charged, interrupting electrolysis. On the whole it was a messy operation.

The neutron yield during this experiment was monitored by the bank of 3 He^3 counters embedded in paraffin. The count rate was initially about 240 counts/10s, comparable to the count rates observed during an initial H_2O electrolysis run. After about 3 hours of D_2O electrolysis the count rate increased slowly to ~ 590 counts/10s. Since no big neutron bursts as in the Pd cathode cells were observed it was suspected that this gradual increase in counts could have been due to amplifier drifts, etc. On switching off the cell current it was noted that the count rate came down to ~385 counts/10s, but it did not quite reach the earlier background levels. When the cell was switched on again however the count rate attained levels of about ~590 counts/s once again. Thereafter operation of the cell was terminated and it was confirmed that the count rate decreased to the original background levels when the cell was removed from the vicinity of the neutron detector.. Throughout this experiment the counts of the plastic scintillator channel monitoring the background did not show any significant variation. In all ~ 3 x 10^7 neutrons were generated during this experiment.

A sample of the electrolyte sent for analysis at the end of the experiment indicated tritium activity of 45 nCi/ml (1.78KBq/ml), a three order of magnitude increase over the initial stock solution value of ~0.05 nCi/ml (1.9 Bq/ml). The net excess tritium produced after correcting for tritium input through make up D_2O addition etc, works out to ~7 μCi or ~1.4 x 10^{14} atoms of T. Admittedly this was not a very clean experiment, but even so one can obtain a very rough value for the neutron–to–tritium yield ratio as ~2 x 10^{-7} for this experiment.

2.5 Pd–Ti PARALLEL PLATE CELL

A simple parallel plate cell with teflon button spacers was fabricated with Pd (0.5 mm thick) and Ti (1 mm thick) plates (40 x 50 mm^2) as electrodes. The inter–electrode gap was ~2 mm. A thin platinum strip was spot welded at the top of the Pd to serve as current feed through. The parallel plate assembly was suspended inside a 300 ml glass bulb having a wide mouth at the top. A vent hole in the stopper permitted escape of electrolytic gases. An advantage of this cell was that either Pd or Ti could be selected as cathode, the other serving as anode.

Electrolysis was commenced on 15th March 1990 with Pd as cathode and 5M NaOD in D_2O as electrolyte. Current density was adjusted to be ~200 mA/cm^2. Three neutron detectors were available for monitoring neutron output, two for viewing the cell and the third for serving as background monitor. Two consecutive neutron bursts occurred about 4 hours after commencement of electrolysis. The background counts was absolutely flat during this run (see Fig.5). It was noted that the Pd cathode had buckled outwards and had become extremely hardened. The buckling can be explained on the basis of differential loading of D_2 across the thickness of the metal.

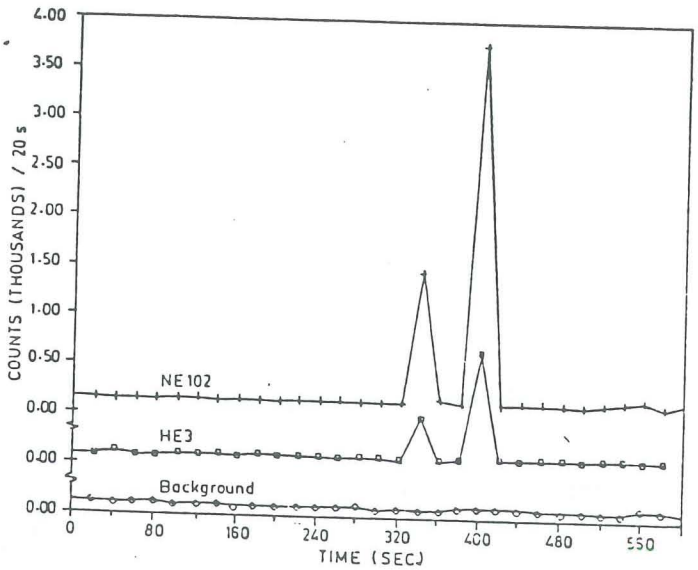

Fig.5 Neutron Burst of Pd–Ti Parellel Plate Cell (15th March 1990)

The Pd cathode was immediately taken for X–ray counting to a low energy NaI detector assembly. Later it was kept overnight in contact with a medical X–ray film for autoradiography. (These techniques are discussed in detail in Ref/5/). However these did not give any evidence

of presence of radioactivity. Samples of the electrolyte taken immediately after the experiment also did not show any significant increase in tritium activity which is indeed very puzzling to the authors. It is possible that the tritium generated had fully escaped along with the electrolytic gases.

3. Pd CATHODE CELLS WITH LiOD ELECTROLYTE

3.1 HOLLOW Pd CYLINDER EXPERIMENT (AnCD)/5/

In this quartz cell the cathode was a hollow Pd cylinder of 1.7 g mass, having a wet surface area of 5.9 cm^2; the anode was a Pt guaze; the electrolyte was 0.1 M LiOD in D_2O (99.87% isotopic purity). To begin with a current of 1 A was used for the electrolysis. After about 30 hours when the temperature attained 60 C, current pulsing between 1 and 2 A at 1 second intervals was adopted. In the absence of a direct neutron detector, this group looked for neutron emission through the 1186 Kev window of the gadolonium capture gamma ray peak. The detector was a 3 x 3 NaI crystal mounted behind a gadolonium compound coated converter plate. After a charge of 17.5 amp–hrs had been passed, the first neutron emission was detected on 21st April 1989. As seen in Fig. 6, in all three distinct neutron bursts of 14

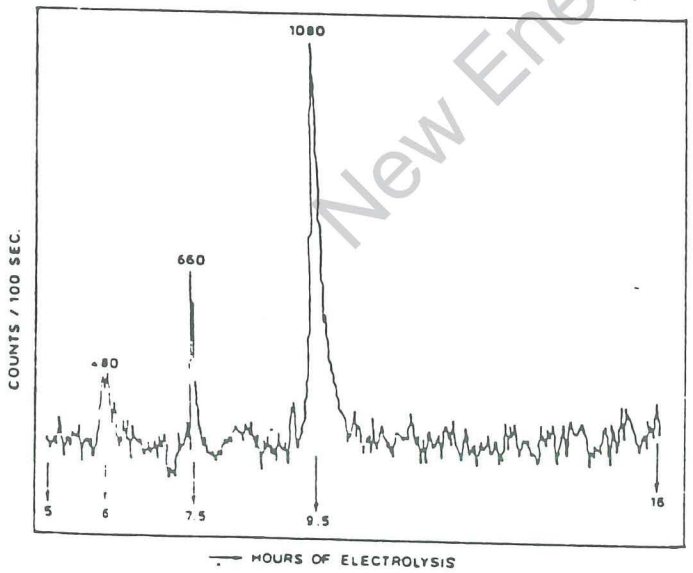

Fig.6 Neutron Yield PDC–I Cell:
1186 Kev Capture gamma Counts

to 20 minutes duration each were produced amounting to an integrated yield of 3 x 10^6 neutrons. Subsequent analysis of a sample of the electrolyte indicated that a total of 3.85 μCi or 7.3 x 10^{13} atoms of tritium had been generated in this experiment. This corresponds to a neutron–to–tritium yield ratio of 4 x 10^{-8}.

3.2 Pd CUBE EXPERIMENT (ROMG)/5/

In this experiment the cathode was a 1 cm^3 of Pd and anode a Pt wire gauze formed into a cylinder surrounding the cube. The electrolyte was 0.1 M LiOD in D_2O. The electrolytic gases were recombined using a Pd catalyst; excess D_2 was converted into D_2O using hot copper oxide. Although the main objective of these experiments was measurement of excess heat, in this paper only the neutron and tritium results are reported.

The neutron yield was monitored by means of a paraffin encased BF_3 counter mounted immediately underneath the table where the cell was located. A second BF_3 detector placed about a metre away monitored background neutrons. The cell electrolyte as well as the D_2O collected in the various cold traps in the system were periodically sampled for tritium measurements. Electrolysis was carried out at a current of ~0.6 amps. After about 24 hrs when ~14.7 amp–hrs of charge had been passed, bursts of neutrons began to be observed. In all 17 neutron bursts lasting from 2 mins to 55 mins each were recorded. The neutron yield in the bursts varied from 5 x 10^3 (2 min burst) to 5 x 10^5 (8 min burst). Altogether a total of 1.4 x 10^6 neutrons was estimated to have been generated. Thereafter there were no more neutron bursts although the electrolysis continued for a further period of seven weeks (a total of 1365 amp–hrs).

A detailed accounting of tritium distributed in various constituents such as electrolyte, vapour condensate recovered from recombined gases, gases extracted from Pd electrode etc was carried out by this group. It was concluded that in all about 35 nCi or 6.7 x 10^{11} atoms of excess tritium was produced in this experiment. This corresponds to a gross neutron–to–tritium yield ratio of 1.7 x 10^{-6}.

3.3 CYLINDRICAL Pd PELLET EXPERIMENT (ROMG)/7/

Here the cathode was a cylindrical Pd pellet 11 mm dia x 11.2 mm height and anode a Pt gauze as before. The concentration of the LiOD electrolyte (120 ml) was increased progressively from 0.1M to ~3M and accordingly the applied voltage decreased, with the current being maintained constant at ~4A. The neutron detection set up was the same as in the previous ROMG experiment. On 13th Feb 1990 when ~3400 amp–hrs had been passed, there was a sharp

burst of 3 x 10^6 neutrons lasting approximately for about 100 s (see Fig.7).

A sample of the electrolyte which was taken the day after this burst, showed a clear eight fold increase in tritium level (64.5 Bq/ml vs preburst value of 7.9 Bq/ml). The tritium level thereafter continuously decreased as shown in Fig.7. But a significant observation was that the rate of decrease was not commensurate with dilution effects caused by make up D_2O addition. The dotted lines commencing from each experimental point in the figure indicates how one would have expected the tritium level to fall if only dilution was playing a role. This implies that additional tritium is continuously entering the electrolyte for many days after the sharp neutron burst. If this is attributed to diffusion of tritium from the inner regions of the pellet, it would support the theory that tritium (and neutron) generation is not restricted to the surface of the cathode alone.

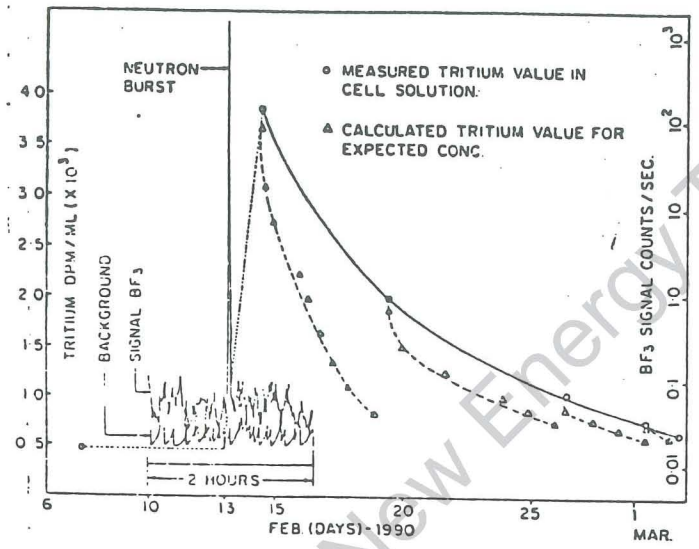

Fig.7. Neutron and Tritium Output of RCS–11 Experiment

After the experiment was terminated following several weeks of electrolysis, the Pd electrode which was found to have developed many cracks on the surface, was degassed at 900 C, and from the volume of D_2 liberated the D/Pd ratio at saturation was deduced to be ~0.85.

3.4 Pd RING AND Pd COIL CELLS WITH NAFION MEMBRANE (ApCD)

In both these experiments the cathodes were thoroughly degassed and vacuum annealed (<10^{-3} mm Hg, 1070 K, ~10 hours) prior to electrolysis. The anode was a cylindrical Pt mesh covering the cathode on all sides. In the first experiment the Pd ring cathode (2.5 cm dia, 1 cm height and 0.1 cm thickness) was charged from both the sides /5/. In the second experiment a thin Pd rod (1 mm dia x 14 cm length) formed into a coil was employed as cathode /8/. In both these experiments the anodes were loosely sandwiched between pairs of Nafion membranes so as to prevent the oxygen evolved at the anode to diffuse back to the cathode surface. The electrolyte (0.1M LiOD in D_2O of 99.86% isotopic purity) was circulated through the quartz electrolytic cell to reduce the dissolved oxygen level further. A saturated calomel electrode dipping in the electrolyte was used to monitor the cathode potential. The cell was operated at a relatively low current density of ~60 mA/cm^2.

Neutron detection was carried out by means of a well type counter containing 24 He^3 detectors embedded in an annular block of paraffin. The test cell was located at the centre of this well, giving a neutron detection efficiency of 8.6%. Data acquisition was carried out with the help of a personal computer having multi–scaling mode facility. The counting time per channel was set as 40s. It was ensured that the overall neutron detection system was immune to extraneous influences which could give false counts. The background reference counts was observed to be steady at ~1.6 cps for about 10 days before start of experiments.

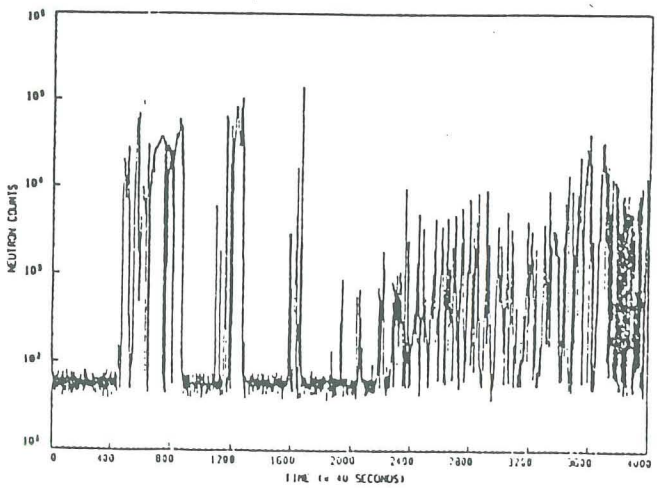

Fig.8 Neutron Bursts of Pd Ring Cathode Experiment of ApCD

The ring cathode electrolysis experiment was run for 32 days commencing from 6th of July 1989. Between the 14th and 17th days from start of electrolysis copious emission of neutrons in the form of bursts was recorded. Otherwise the count

rate remained close to background levels for the rest of the period. Fig. 8 depicts the neutron counts variation over the entire 44 hour (4000 x 40s) duration, while Fig 9 gives an expanded view of a part of the same data. The log scale of the counts axis should be noted, indicating that the bursts were indeed intense, the peak neutron emission rate being ~1000 times background levels. The total neutron emission over the 40 hour neutron active phase was 1.8×10^8 neutrons. It is interesting to note however that even during an intense phase of neutron production the count rate suddenly dropped to near background levels and remained so for several seconds before abruptly climbing back to levels over a 100 times the background value.

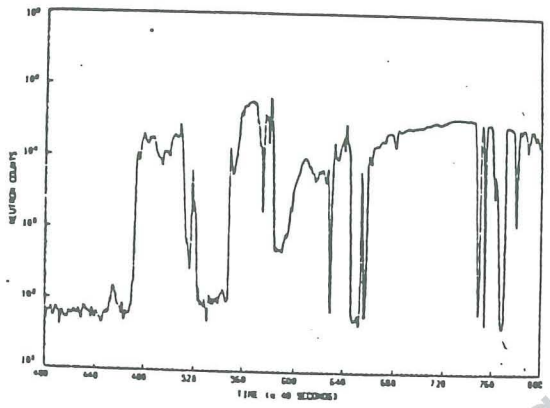

Fig.9 Expanded View of Portion of Fig.8

The electrolyte was sampled once in 6 days during the experiment and analyzed for tritium content using standard liquid scintillation techniques. After the neutron active phase, the tritium level of the electrolyte showed an increase from 0.4 to 1.3 Bq/ml. The cell electrolyte volume being 250 ml, this corresponds to an excess tritium generation of 1.8×10^{11} atoms. This does not include the tritium carried away by the gas stream. Degassing of the Pd cathode at 680 K and reformation of water over hot CuO turnings yielded an additional 3×10^9 tritium atoms only. Thus an upper limit to the overall neutron–to–tritium yield ratio in this experiment works out to ~10^{-3}.

The electrolysis experiment with the Pd coil cathode was carried out for 24 days. The electrolyte was 0.05M Li_2SO_4. Neutron emission started within 4 hours of commencement of electrolysis and lasted for a total period of 15 hrs spread over the first five days of the electrolysis. The integrated neutron yield was 5.8×10^6 while the tritium yield at the end of the experiment amounted to 1.8×10^{10} atoms. The neutron–to–tritium yield ratio in this experiment works out to be 3.2×10^{-4}.

It is significant that the neutron to tritium yield ratios of 10^{-3} to 10^{-4} observed in both these experiments are several orders of magnitude larger than the values of 10^{-6} to 10^{-8} obtained in all the previous cells. One possible explanation could be that the employment of Nafion membrane in conjunction with the very low levels of dissolved oxygen in these cells might have been responsible for preventing recombination of T back into DTO and instead have allowed most of the T to escape along with the gas stream. As noted earlier the tritium carried away by the gas stream was not measured in this experiment also.

4. SUMMARY OF ELECTROLYSIS EXPERIMENTS WITH NEUTRON AND TRITIUM GENERATION

Table I presents a summary of the successful electrolysis experiments conducted so far at Trombay wherein significant amounts of both neutron and tritium production has been observed. Also included (last column of Table I) for comparison and completeness are the results of an experiment /9/ conducted at the Indira Gandhi Centre for Atomic Research (IGCAR) in Kalpakkam, Tamil Nadu, a sister Institution of BARC. The main conclusions to emerge out of these results are discussed later.

5. OTHER TRITIUM PRODUCING CELLS / EXPERIMENTS

Besides the above electrolysis experiments wherein both tritium and neutron production has been observed, there have been an additional 11 cells / experiments wherein clear evidence for excess tritium generation has been obtained. Majority of these experiments were carried out in the various divisions belonging to the "Chemical Group " of BARC. Table II summarises these results. In most of these experiments neutron yield, if any, was not monitored due to non–availability of detectors with the groups concerned. In the few cases where neutron detectors were present the increase in count rates if any was not significant enough within the statistics of the background count rate variations. Some of the cells of Table II were of closed type wherein the electrolytic gases were recombined by means of a suitable catalyst.

The MR(Jr) cell was a smaller version of the Milton Roy cell with 6 Pd–Ag alloy tubes. This cell has earlier been used for routine H_2 generation for several years at the Chemistry

TABLE I: SUMMARY OF ELECTROLYSIS EXPERIMENTS WITH NEUTRON AND TRITIUM GENERATION

Sr. No.	#1	#2	#3	#4	#5	#6	#7	#8	#9	#10	#11
Division	DD/ HWD NtPD	NtPD/ HWD	NtPD/ HWD	DD/ HWD NtPD	HWD/ NtPD	AnCD	ROMG	ROMG	ApCD	ApCD	IGCAR
Cell (Name)	Ti–SS	MR–1	MR–2	5 Module	Par. Plate	PDC–I	RCS–11	RCS–19	Nafion–1	Nafion–2	RCP–II
Date	1989 21 May	1989 21 April	1989 12–16 Jun	1989 5 May	1990 15 March	1989 21 April	1989 June–Aug.	1989 Jan.–Apr.	1989 July	1990 Feb.	1989 Dec.
Cathode:											
Material	Ti	Pd–Ag	Pd–Ag	Pd–Ag	Pd	Pd	Pd	Pd	Pd	Pd	Pd
Geometry	Rod	Tubes	Tubes	Discs (5)	Plate	Hollow Cyl.	Cube	Pellet	Ring	Coil	Button
Dimensions (in mm)	22 ϕ x 150 long	← 3 o.d. x 200 ht →		115 ϕ x 0.1 thk	40 x 50 x 1 thk	–	1 cm^3	11 ϕ x 11.2 ht.	25ϕ x 10 ht x 1 thk.	1 ϕ x 140 long	–
Area (Cm^2)	104	300	300	78	20	5.9	6	5.7	18	4.4	8
Anode	S.S. Pipe	Ni–Pipes	Ni–Pipes	Porous–Ni	Ti Plate	Pt Mesh	Pt Mesh	Pt Mesh	Pt Mesh	Pt Mesh	Pt Mesh
Electrolyte	NaOD (5M)	NaOD (5M)	NaOD (5M)	NaOD (5M)	NaOD (5M)	LiOD (0.1 M)	LiOD (0.1 M)	LiOD (0.1 M)	LiOD (0.1 M)	Li_2SO_4 (.05 M)	LiOD (0.1 M)
Volume (ml)	135	250	250	1000	300	45	150	120	250	140	–
Current Density (mA/cm^2)	≤400	~300	~300	~800	~200	≤340	~100	~700	~60	~50	<100
Switching On:											
Charge ($\frac{A-hrs}{cm^2}$)	1.2	0.6	–	3.2	0.8	3.0	2.5	650	3.4	0.15	36.7
Time (hrs)	3	5	0.5	4	4	9	24	930	560	3	300
Active Life	Few hrs	~3.5 hrs	~2 hrs	≤3 mins	<1 min	~5 hrs	~5 d	~100 sec	~40 hrs.	~5 d	8 hrs
Neutron Yield:											
No. of Bursts	Continuum	9	1	1	1	3	17	1	Many	Many	2
Total	$3x10^7$	$4x10^7$	$.9x10^7$	$5x10^6$	$1x10^6$	$3x10^6$	$1.4x10^6$	$3x10^6$	$1.8x10^8$	$5.8x10^6$	$2.4x10^6$
n/cm^2	$2.9x10^5$	$1.7x10^5$	$1.3x10^4$	$1.3x10^4$	$5x10^4$	$5x10^5$	$2.3x10^5$	$5.2x10^5$	10^7	$1.3x10^6$	$3x10^5$
Tritium Yield:											
Total (Bq)	$2.6x10^5$	$1.5x10^7$	$3.8x10^6$	$7x10^6$	–	$1.42x10^5$	$1.3x10^3$	$7.7x10^3$	325	32.5	$6.3x10^3$
Total (Atom)	$1.4x10^{14}$	$8x10^{15}$	$1.9x10^{15}$	$4x10^{15}$	–	$7.2x10^{13}$	$6.7x10^{11}$	$4x10^{12}$	$1.8x10^{11}$	$1.8x10^{10}$	$3.5x10^{12}$
t/cm^2	$1.3x10^{12}$	$2.7x10^{13}$	$6x10^{12}$	10^{13}	–	$1.2x10^{13}$	$1.1x10^{11}$	$5.2x10^{13}$	$1x10^{10}$	$4x10^9$	$4.4x10^{11}$
(n/t) Ratio	$2x10^{-7}$	$0.5x10^{-8}$	$0.5x10^{-8}$	$1.2x10^{-9}$	–	$4x10^{-6}$	$1.7x10^{-6}$	10^{-6}	10^{-3}	$3.2x10^{-4}$	$7x10^{-7}$

TABLE II: SUMMARY OF OTHER TRITIUM PRODUCING ELECTROLYTIC EXPERIMENTS

Sr. No.	#1	#2	#3	#4	#5	#6	#7	#8	#9	#10	#11
Division	Heavy Water Divn.		ROMG	Analytical Chemistry Division					Chemistry Division		
Cell (Name)	MR (Jr)–I	MR (Jr)–II	RCS–18	PDX–0	PDC–II	PDC–III	PDC–IV	PDR–I	CD–4	CD–6	CD–5
Date	1989 21 Sept.	1990 5 March	1989 24 Oct.	1989 24 April	1989 10 July	1989 6 Sept.	1989 29 Sept.	1989 9 Nov.	1989 21 July	1989 22 Dec.	1989 24 Oct.
Cathode:											
Material	–Pd–Ag Alloy–		Cold Rolled Pd	Pd	Pd			Pd	Pd ingot	Pd	Pd wire
Geometry	Tubes		Hollow Cyl.	Ring	Hollow Cylinder			Rod	Cylinder	Pellet	Grid
Dimensions (in mm)	3 ϕ x 150 ht		–	2 thk.	–	–	–	4 ϕ x 19 long	8 ϕ x 16 long	4 ϕ x 4 ht.	0.5 ϕ x 800 long
Area (Cm^2)	113		19	14.5	6.37			2.75	0.57	0.126	4
Anode	Ni Pipe		Pt gauze	Pt Discs	Pt Mesh	Pt Mesh	Pt Mesh	Pt Mesh	Pt Coil	Pt Wire	Pt Wire
Electrolyte	NaOD (5M)		LiOD (0.1 M)	LiOD (0.14 M)	LiOD (0.1 M)	LiOD (0.1 M)	LiOD (0.14 M)	LiOD (0.1 M)	LiOD (0.1 M)	LiOD (0.1 M)	KOD (Cone) (Paste)
Volume (ml)	150		150	65	60	100	80	80	28	3	1.5
Open/ Closed	Open		Open	Closed	Open	Open	Closed	Closed	Open	Semiopen	Closed
Current (A)	40		~ 2	≤ 2	1~2 Pulsed	1~3 Pulsed	1~2 Pulsed	0.2~2.2 RF Superposed	100 mA	≤ 35 mA	30 mA
Current Density (mA/ cm^2)	350		~105	~160	<350	≤470	<350	<800	~100	~278	7.5
Duration of Electrolysis	12 hrs	30 hrs	13 d	7.4 d	366.2 hrs	183.3 hrs	5.8 d	40 d	190 d	17 d	80 d
Tritium Measurements:											
Initial Concn. (Bq)	1.44	3.33	3.6	2.7	2.81	2.77	2.70	2.68	4.6	2.0	2.5
Maximum Concn. (Bq/ml)	225.7	18.5	–	0.93×10^4	5.88×10^4	4.6	–	–	72.1	65.0	22.9
Output to Input Ratio	156.7	5.6	3.36	3425	20,925	1.66	2.5	1.91	15.7	32.5	9.16
Net Excess:											
(Bq)	3.3×10^4	2.28×10^3	2.71×10^3	6.02×10^5	2.08×10^6	2.96×10^3	6.29×10^2	1.1×10^3			
(Atoms)	1.76×10^{13}	1.2×10^{12}	1.44×10^{12}	3.2×10^{14}	1.1×10^{15}	1.56×10^{12}	3.96×10^{11}	5.83×10^{11}	10^{12}	10^{11}	2×10^{10}
t/ cm^2	1.6×10^{11}	1.1×10^{10}	0.8×10^{11}	2.2×10^{13}	1.7×10^{14}	2.4×10^{11}	6.2×10^{10}	2.12×10^{11}	1.8×10^{12}	0.8×10^{12}	0.5×10^{10}

Division. Cell #11 was a very novel cell wherein the electrolyte was KOD in the form of a paste applied on a multiwire grid electrode system made of alternate wires (0.5 mm dia) of Pd and Pt. Although this gave excess tritium of only 2 X 10^{10} atoms (lowest in the Table) it must be noted that the total inventroy of electrolyte in this microcell was hardly 1.5 ml. In Cell #8 (PDR–I) an RF voltage was superposed on the applied DC voltage with a view to ascertain whether this would help improve the tritium production. As seen from the results there is no evidence of any improvement.

The Cells # 5, 6, & 7 used the same hollow Pd cylinder cathode deployed in Cell # 6 of Table I (Expt. PDC–I). As in the case of PDC–I current pulsing was resorted to in these three PDC series of experiments also. On completion of PDC–I the hollow Pd cathode was degassed at 300 C in a vacuum furnace for over 2 hrs. Subsequent electrolysis in 0.1 M LiOD (Expt. PDC–II) terminated in an explosion after a charge of 423 amp–hrs had been passed. Careful measurements of the tritium content indicated that over 2 MBq (1.1 x 10 ^{15} atoms) of excess tritium had been generated during this experiment. (see Table II). This corresponds to an increase in tritium inventory by a factor of more than 20,000 relative to the total tritium input to this experiment.

This same cathode generated tritium two more times after degassing and reuse. (PDC–III & IV). But as seen from Table II in each subsequent run the quantum of tritium generated decreased further. For example in PDC–III while excess tritium recovered was 2.96 KBq, in PDC–IV the excess tritium was only 629 Bq even after 123 amp–hrs of charging. Prior to commencement of PDC–IV the electrode had been heated to 850 C for 4 hrs in vacuum, cooled and again heated to 800 C in D_2 gas atmosphere at 1 cm pressure for 3 hrs followed by degassing again under vacuum for 3 hrs. This very elaborate pretreatment would have cleansed the Pd of any remnant tritium within its interior, confirming that the fresh amount 629 Bq obtained in PDC–IV must have been generated during this run of electrolysis only. But the more important implication of this result is that even vacuum heating–annealing does not appear to have restored the ability of the Pd cathode to support nuclear reactions.

6. REAL TIME NEUTRON DIFFRACTION STUDY OF DEUTERON LOADING IN A Pd CATHODE /11/

Cell #9 (CD–4 of Chemistry Division) was primarily designed for conducting an online neutron diffraction study of the phase changes occurring during the deuteration of a Pd rod. It comprised of a covered pyrex glass beaker with an 8 mm dia x 16 mm long Pd cathode of which 11 mm protruded underneath the cell and was set up in front of one of the neutron beams at the Dhruva research reactor at Trombay. The portion of the Pd rod exposed to atmosphere was given thin protective coating of tin to minimise escape of deuterium. A platinum cell above the cathode served as anode and the electrolyte was 0.1M Li_2O in D_2O. A 0.18 mm thick Nafion membrane between the electrodes helped prevent direct recombination of deuterium and oxygen. Electrolysis was carried out at a steady current of 100 mA extending over a period of more than 8 weeks. Powder diffraction patterns were recorded periodically with the help of a 1 metre wide position sensitive neutron detector mounted so as to provide a 30^0 angular span. With this arrangement the real time development of the (111),(200) and (311) reflections could be studied.

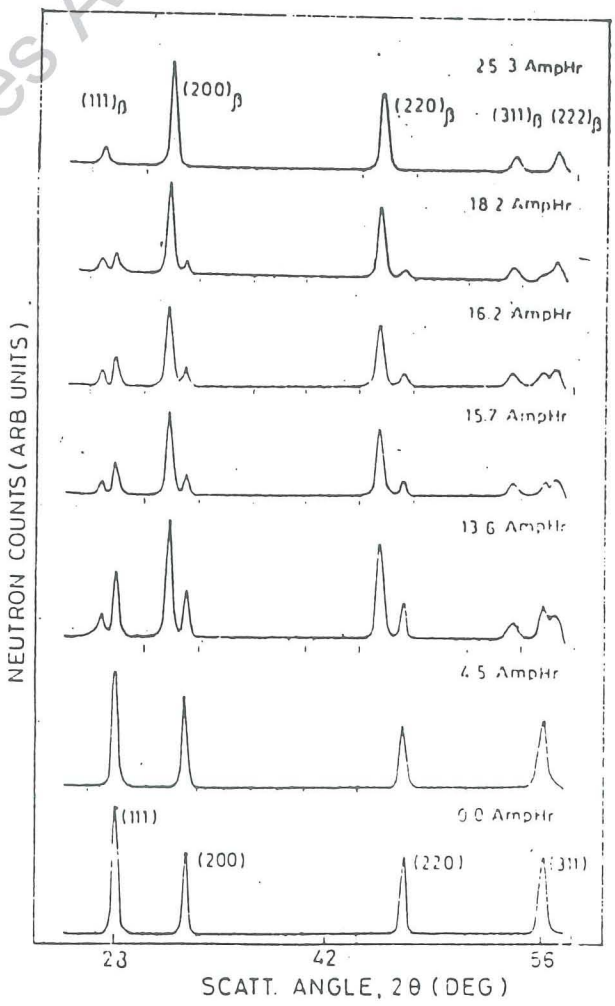

Fig.10 Neutron Diffraction Patterns at Different Deuterium Loadings of Palladium Rod

Fig.10 displays the recorded diffraction patterns at various loading stages measured in terms of amp–hrs of electrolysis. It took an hour's reactor time for each spectrum. The Pd electrode was initially in pure fcc(metallic) form with the lattice constant of 3.89 A^0 characterized by the four peaks corresponding to α phase seen in the bottom most pattern of Fig.10. New peaks indicative of the precipitation of the β phase of the Pd–D system (lattice constant of 4.02 A^0) showed up at around 10 amp–hrs of electrolysis. As electrolysis proceeded the intensity of the β phase peaks built up at the cost of α phase peaks. It was found that within about 25 amp–hrs the upper part of the Pd electrode projecting underneath the cell was completely converted to β phase wherein the lower part required nearly 60 amp–hrs for this. From the ratio of the structure factors of the odd to even reflections (S(111)/S(200)) the stochiometry in the β phase could be deduced. It was summarised from the study that the stochiometry at saturation was $PdD_{0.55}$. The main conclusion of this experiment relevant to cold fusion is that no new phases develop in Pd even after 100 amp–hrs of electrolysis.

PART B: D_2 GAS LOADING EXPERIMENTS

7.1 SEARCH FOR TRITIUM IN GAS LOADED Pd SAMPLES/5/

In these experiments D_2 gas was loaded into Pd samples after thoroughly degassing them and a search was made for the possible production of tritium in the samples. The tritium produced if any along with that in the initially loaded deuterium was extracted through isotopic exchange with distilled light water wherein the Pd sample itself served as a catalyst. From the activity measured in the water the amount of tritium "produced" in the Pd was computed.

D_2 gas was generated by reducing D_2O with Na in vacuum and stored under pressure in an SS dewar with liquid nitrogen cooling, in the presence of activated charcoal. The stock D_2O used had a tritium content of 0.075 nCi/ml (2.8 Bq/ml), corresponding to a (T/D) isotopic ratio of 3×10^{-14}. Pd samples either in the form of Pd black powder or Johnson & Matthey Pd–Ag foils were taken in an SS reaction vessel connected to a vacuum system (10^{-5} mm) through a buffer tank of 1 litre volume equipped with a pressure gauge. After degassing and cooling under vacuum, D_2 gas at 1 atm pressure was let into the buffer tank and the system sealed off for equilibration with the Pd contained in the reaction vessel for several hours or days at times. From the pressure drop observed the quantity of gas absorbed in the Pd could be deduced. The deuterated Pd samples were later immersed inside a measured quantity of distilled water for a few hours and the concentration of tritium in the water measured through standard liquid scintillation counting systems. The tritium content in the Pd was deduced therefrom knowing the gram moles of D_2 absorbed in Pd as well as the relevant equilibration constant (K).

TABLE III
TRITIUM PRODUCTION IN D_2 GAS LOADED Pd SAMPLES

Experiment No	#1	#2	#3	#4	#5
Nature of sample	Pd black powder	Pd–Ag foil	Pd–Ag foil	Pd–Ag foil	Pd–Ag foil
Mass (g)	20	0.96	10.9	10.6	0.43
Date of loading	20 June 89	24 Aug 89	19 Sept 89	7 Mar 90	19 Sept 89
Volume of D_2 absorbed (ml)	1325	34.5	516.4	222	22.2
(D/Pd) ratio	63	46	45	20	45
Equilibration time (hrs)	16	16	240	40	240
Water used for extraction (ml)	50	5	50	50	5
Tritium activity of water (bq/ml)	6.1	5.9	8.5	12.5	32.6
T/D ratio in Pd	32×10^{-11}	1.1×10^{-11}	$.67 \times 10^{-11}$	3.4×10^{-11}	8.3×10^{-11}
Absolute tritium activity (bq)	410.7	37.0	429.2	717.5	159.1
Total tritium atoms in Pd	2.31×10^{11}	2.02×10^{10}	2.4×10^{11}	4.1×10^{11}	8.96×10^{10}
Tritium atom per g of Pd	1.2×10^{10}	2.1×10^{10}	2.2×10^{10}	3.6×10^{10}	20.8×10^{10}

Table III summarizes the results. The tritium activity measured in the distilled water was a small fraction of a nCi/ml (5 to 30 Bq/ml). The total quantity of tritium estimated to have been generated in the Pd foils is in the region of 10^{10} to 10^{11} atoms. It is observed that the (D/Pd) ratios attained following D_2 absorption are approximately similar in all the cases (0.20 to 0.63). The amount of tritium produced per gram of Pd sample varies widely, from ~1.2 to 20.8 x 10^{10} atoms/g. As may be expected the higher value is consistent with the longer duration of equilibration time (240 hours) between D and Pd, but the large Pd foil (column #3) which was also equilibrated for 240 hours has given only 2.2×10^{10} atoms of t/g of Pd. In all cases the finally attained (T/D) ratios which are in the range of $.3 \times 10^{-11}$ to 8.3×10^{-11} are two to three orders of magnitude higher than that of the initial gas value namely ~ 3×10^{-14}. Thus fresh tritium amounting to about 10^{10} or 10^{11} atoms appears to have been created in the Pd, presumably due to "cold fusion" reactions. It is not clear whether the tritium was produced during the absorption process or during the subsequent "curing" or equilibration phase.

The presence of tritium in the Pd–Ag foils has also been independently confirmed through autoradiography. Fig 11 shows the radiograph of a triangular Pd–Ag foil.The image displays some non–uniformity in fogging. It was however observed that the intensity of fogging of these Pd–Ag foils rapidly decreased when attempts were made to reproduce the radiographs on subsequent days, indicating that the tritium retention capability of Pd–Ag is not as good as that of titanium.

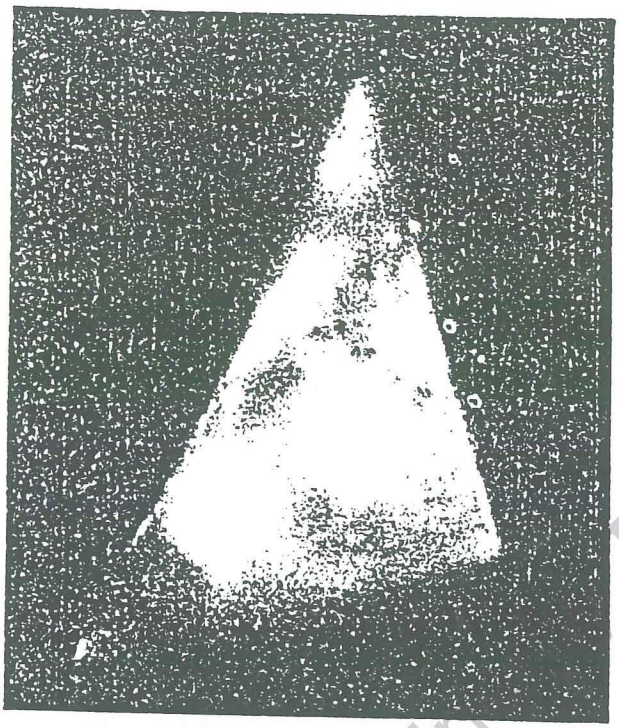

Fig.11 Autoradiograph of Deuterated Triangular Pd–Ag Foil

7.2 FRASCATI TYPE EXPERIMENTS WITH Ti SHAVINGS (CD)/5/

A readily available set up employed earlier for high pressure hydriding studies was used for these experiments/12/. Ti metal pieces cut from a sheet were surface cleaned and subjected to activation treatment prior to loading in the high pressure cell. D_2 gas pressure or temperature was cycled between high and low values using liquid nitrogen. The well type neutron counter employing 24 He^3 detectors embedded in paraffin, along with the associated data acquisition system described in Sec 3.4 was used for the neutron yield measurements. The counting efficiency was determined to be 10%.

In these experiments first conducted in June 1989, Ti pieces were to begin with equilibrated with D_2 gas at 10 atm and 77 K for ~20 minutes. The temperature was then allowed to increase slowly to ambient level, with simultaneous evacuation resulting in desorption of D_2 gas from the Ti shavings. This resulted in large neutron bursts lasting between half an hour to ~2 hours each as shown in Figs. 12a to 12d. Prior to the experiments of Figs 12c & 12d, the D_2 gas pressure and temperature were simultaneously cycled. While the first three measurements were carried out with the same charge of Ti, the last one was done with a fresh charge which could be the reason for the slightly different characteristics of Fig. 12d. The integrated neutron yield in these experiments varied in the range of 10^5 to 10^7 neutrons.

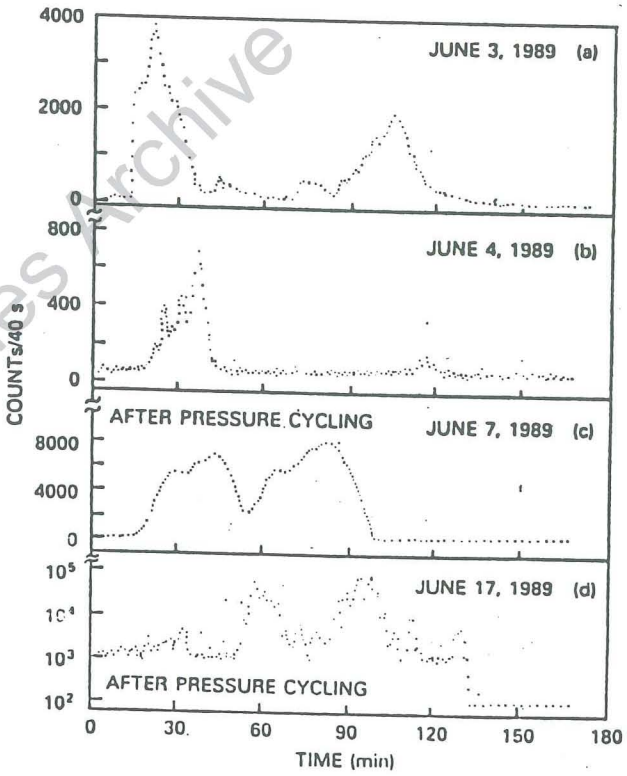

Fig.12 Neutron Counts Variation During Frascati Type Experiment with Ti Shavings

The authors of these measurements have reportedly been unsuccessful in their attempts to measure neutrons again in repeated attempts to reproduce the earlier results /13/. However they have detected the presence of tritium in some of the newly loaded Ti shavings through the technique of autoradiography. For this they employed a high speed polaroid camera. A 25 μm thick aluminium foil placed between the shavings and the photo sensitive film ensured that false images are not produced due to mechanical scratching of the film by the sharp edges of the shavings. The aluminium foil converts the tritium betas into soft X–rays which emerge from the foil

giving rise to image formation.(see Sec. 8.3).

7.3 EVIDENCE FOR TRITIUM IN Ti TARGETS SUBJECTED TO RF HEATING IN D_2 ATMOSPHERE (TPPED/NtPD) /5/

In these experiments machined and chemically cleaned targets of Ti were individually heated to temperatures of upto 900 C in a glass chamber using a surrounding induction heating coil (1 to 2 MHz frequency, 3 to 6 KW power). The glass chamber was connected to a vacuum system as well as H_2 and D_2 gas bottles. Degassing was initially carried out at 900 C for several hours until a vacuum of 10^{-5} mm was maintained steadily. The targets were later heated to 600 C in H_2 atmosphere at a few mm of pressure. The induction heater was then switched off and the target allowed to cool absorbing H_2 in the process. Atleast three cycles of H_2 absorption /desorption was given "to create active sites for D_2 absorption". Three such heating/cooling cycles were then carried out with D_2 gas. Pressure drop recorded by an oil manometer indicated the quantity of gas absorbed during each cooling cycle. It was observed that the quantity of gas absorbed increased each time saturating in the 3rd or 4th cycle.

The targets typically absorbed $<10^{19}$ molecules of D_2. Since the mass of Ti was a few hundred milligrams, this corresponds to a gross (D/Ti) ratio of hardly 0.001. However we have reason to believe that most of the absorption would be confined to the surface region. This is because when a metallic object is heated by induction heating the current distribution falls off exponentially with increasing depth. The skin depth δ characterizing this phenomenon is given by $\delta^2 = (s/f\pi\mu)$ where s and μ are the resistivity and permeability respectively of the workload and f is the frequency of the applied electromagnetic field. For a frequency of 1 to 2 MHz, δ for Ti is 0.1 mm. Consequently we believe that the (D/Ti) ratio in the near surface region would be much higher than the gross value of 0.001 noted earlier.

After loading, all the targets were subjected to various tests such as autoradiography, K X–ray counting etc in search of tritium. Although several dozen targets were successfully loaded with D_2 gas, only a few of them gave positive evidence for the presence of tritium. Interestingly the samples which soaked up large amounts of D_2 gas did not give any positive results. The best results were obtained from a disc shaped button (10 mm dia x 2 mm thick) and a couple of conical pieces meant for use as electrodes. Table IV summarizes the results. Figs. 13 and 14 are the autoradiographs of a deuterated disc and cone respectively. The radiographs of the Ti disc shows about 50 to 60 spots randomly distributed within the boundary. The occurrence of spots all along the rim of the machined disc is very intriguing. It is estimated

TABLE IV

TRITIUM CONTENT IN D_2 GAS LOADED Ti TARGETS

Date of loading	14 June 89	9 June 89	21 Mar 90
Shape of sample	Disc	Cone	Cone
Sample mass (g)	.98	.206	.2
D_2 absorbed (mg)	.42	.07	.29
T activity from X–ray counts(bq)	290	1300	5.5×10^{6}
Date of counting	16 June 89	16 June 89	28 Mar 90
Tritium atoms	1.5×10^{11}	6.5×10^{11}	3.0×10^{11}
T/D ratio	1.2×10^{-9}	3.2×10^{-8}	7.1×10^{-5}

that each spot corresponds roughly to 10^9 to 10^{10} atoms of tritium. The total number of tritium atoms in the whole target works out to be $\sim10^{11}$. This is to be compared with the 10^{19} to 10^{20} atoms of D absorbed in all by the Ti, pointing to a gross (T/D) ratio of $>10^{-9}$.

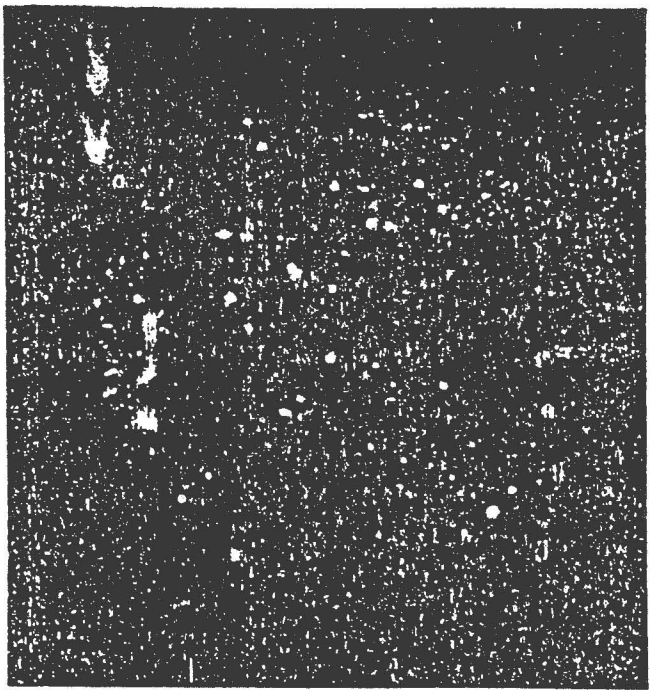

Fig.13 Autoradiograph of Deuterated Ti Disc

Interestingly one of these disc targets which was kept in front of a paraffin encased BF_3 neutron detector and monitored over a week end,

suddenly emitted a large burst of neutrons on its own lasting over a time span of 85 mins. (See Fig.2 of companion BARC paper /6/)
The most intriguing feature of this experiment is that ever since the RF heating system became defective in September 89, these results could not be reproduced even once using a resistance furnace

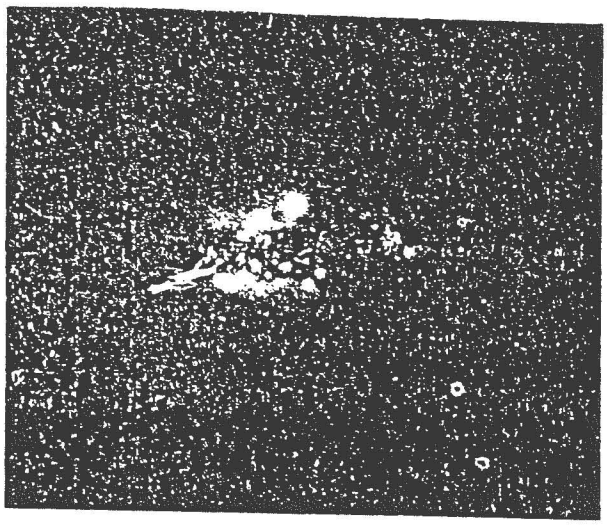

Fig.14 Autoradiograph of Deuterated Ti Cone

even though the power was higher and much greater quantities of D_2 gas could be absorbed. It was only in March 90 when a 1MHz furnace became available that a conical target once again indicated the presence of μCi amounts of tritium (See Table IV). The authors are therefore obliged to conclude that oscillatory electric and magnetic fields somehow play a crucial role in stimulating anomalous nuclear reactions on the surface of these machined Ti targets. In this context the recent Coherent Theory of Cold Fusion proposed by Hagelstein /14/ appears to give some theoretical insight as to possible mechanisms which could explain these observations.

7.4 ANOMALOUS TRITIUM PRODUCTION IN TITANIUM ELECTRODE OF A PLASMA FOCUS DEVICE (NtPD)/15/

A Plasma Focus (PF) device forms a high density (10^{25} ions/m^3), high temperature (10^7 K) plasma which produces an intense burst of neutrons when operated with deuterium gas/16/. This device has a vacuum chamber consisting of coaxial cylindrical electrodes connected through a Corning glass insulator at the bottom end and left open at the top. Fig.15 shows a schematic diagram of a plasma focus device of the type used in the present experiments. When this coaxial gun is connected to a high voltage (15 to 50 KV) capacitor bank with the help of a spark gap switch, a surface discharge is initiated at the insulator end. This then develops into a radial current sheath which is accelerated by J x B forces down the length of the electrode system, sweeping the gas ahead of it. On reaching the open end, the current sheath turns around on itself forming a quasi–cylindrical implosion resulting in a dense hot "plasma focus" a few cm long and few mm in diameter just above the tip of the central anode.

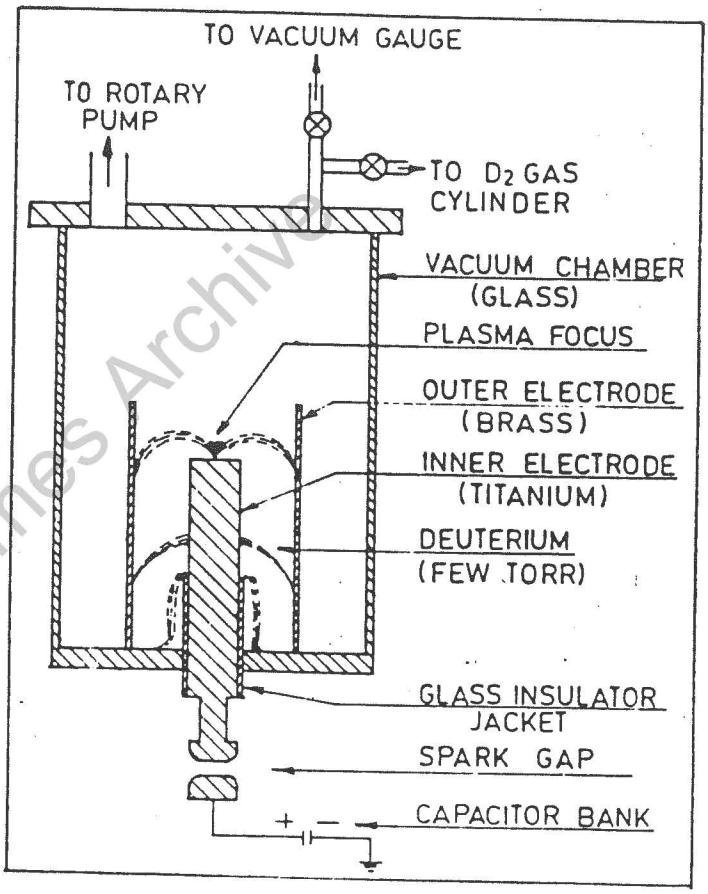

Fig.15 Schematic Diagram of a Plasma Focus Device With Central Ti Electrode

During January 90 an experimental program was underway to study the influence of anode material on the characteristics of the plasma created and consequently on the neutron production mechanisms of a PF device. Brass, aluminium, stainless steel and titanium were investigated for neutron yield systematics under various deuterium filling pressures (1–10 mbar).

During the experiments with a Ti anode, about 80 charge/discharge shots were carried out. After each shot the chamber was flushed and filled with fresh D_2 gas. Some of these shots were performed with the central electrode operated

with negative polarity. This mode of operation would direct the deuteron beams of a few hundred KeV energy which are known to be generated in PF devices, towards the central electrode. The neutron yield in every shot was recorded with the help of a bank of calibrated activation type silver cathode Geiger Muller counters located close to the device. Typically with ~3 KJ of stored energy, 10^7 neutrons were produced in each normal shot i.e. when the central electrode is used as anode. In the polarity reversed mode of operation also neutrons were produced but the magnitude of the neutron burst was an order of magnitude smaller.

In view of the special role of Ti in D_2 gas loaded cold fusion experiments, the Ti electrode was tested for induced radioactivity using autoradiography immediately after the experiment on two consecutive nights (4^{th} and 5^{th} Jan.90). No image was found at that time. But five weeks later (on 9^{th} Feb 90) using a new NaI detector set up it was discovered that a surprisingly high activity of ~392 μCi (~ 10^{16} atoms of tritium) had built up on the surface of the Ti electrode which had been exposed to the plasma focus shots. To confirm this, the rod was subjected to overnight autoradiography once again. As seen in Fig. 16 a very beautiful and impressive image was obtained.

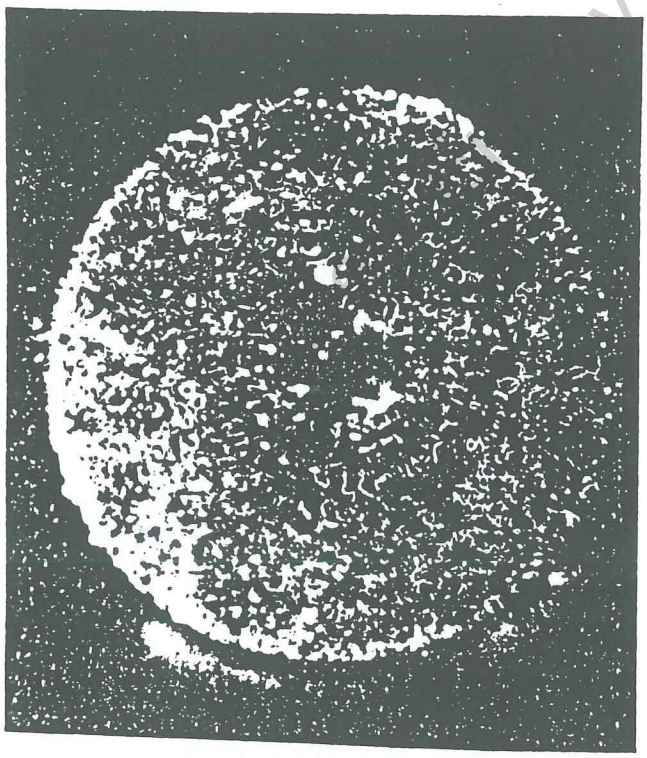

Fig.16 Autoradiograph of Central Ti Electrode of PF Device (5 Weeks After Charging)

This image has since been reproduced several times through repeated autoradiographic exposures. There has been no change in the resolution or clarity of the images even after a gap of a few months. The very sharp worm like lines are believed to be due to β^s from tritium bearing grain boundary regions near the surface, while the intense diffuse spots are due to soft X–rays (Ti K X–rays) emanating from deeper layers of the titanium electrode. As in the case of the RF heated Ti disc autoradiographs (Fig 13) the presence of spots all along the periphery of the rod is noteworthy. In order to rule out the possibility of image formation due to tritium contamination in the D_2 gas, the other electrodes (Al, SS and brass) were also tested for radioactivity. But none of them showed any activity confirming that the phenomenon is unique to Ti.

During all the 80 PF shots it is estimated that atmost 10^9 (d–d) neutrons could have been generated due to hot fusion reactions. Since in conventional (d–d) reactions (both beam target and hot fusion) the neutron–to–tritium branching ratio is ~ 1, it follows that not more than 10^9 tritium atoms could therefore have been produced during the shots. It is totally unrealistic to expect or postulate that all this tritium would have succeeded in getting absorbed on the Ti anode surface. Even if that were true, it still can not explain the 10^{16} atoms of tritium measured on the tip of the Ti anode. But the even more intriguing question is why was it not seen in the radiographs taken on the same night of the experiment as well as on the following night? Although the presence of large amounts of tritium was first detected only five weeks later, it is possible that it might have been produced any time in the intervening period.

The authors suggest that the intense electric and magnetic fields involved in the operation of a PF must have had some role to play in causing "cold fusion" reactions on the tip of the anode. Repeat experiments using planchets of Ti mounted at the top of a brass anode have however not shown any activity so far. A fresh stock of pure D_2 gas as well as a new Ti electrode are awaited for repeating the experiments under identical conditions prevalent in the earlier successful experiment.

7.5 OBSERVATION OF HIGH TRITIUM LEVELS IN AGED DEUTERATED Ti TARGETS/5/

The Division of Radiological Protection of BARC had procured a number of deuterated titanium targets on copper backing during 1972 to 1981 for dosimetry studies with accelerator based neutron sources. Twelve such targets were available, nine procured from M/S Amersham

International of U.K. and the remaining three from the Isotope Division of BARC. In view of the various studies involving deuterated titanium targets described in the earlier sections, it was conjectured that cold fusion reactions might have occurred in these "aged" targets over the past 9 to 18 years and if so, it was argued, they should contain considerable amounts of tritium. In order to check this hypothesis these aged targets were subjected to various studies for establishing the magnitude of tritium in them. Five different techniques namely autoradiography, Ti K X–ray counting with NaI and high purity germanium detectors, β counting with proportional counters and current measurements with an ionization chamber were used. The details of the targets, measurements and results are described in Ref /5/. It was found that the absolute tritium content in the targets varied between 0.3 and 150 MBq. Inquiries with the suppliers of these targets indicate that while inadvertent contamination during manufacture to the extent of a few hundred Bq is in principle a likely possibility, contamination levels in the MBq region is difficult to explain. The tritium levels in these aged TiDx targets expressed in terms of the (T/D) isotopic ratios was seen to vary in the range of 0.07 to 3.5 x 10^{-4}. For comparison the tritium activity of the D_2O moderator of a CANDU type power reactor is atmost 30 Ci/l even at saturation, corresponding to a (T/D) ratio of 10^{-5}. In contrast the (T/D) ratio of fresh D_2O from a factory is typically in the region of 10^{-14} to 10^{-13} only. Hence the authors are inclined to speculate that a plausible explanation for the unexpectedly high tritium levels in aged deuterated Ti targets could be the occurrence of cold fusion reactions..

8. MEASUREMENT OF TRITIUM LEVELS IN AQUEOUS AND METALLIC SAMPLES

As a consequence of the many years of operational experience with heavy water moderated research and power reactors in India, considerable expertise has been built–up in the area of tritium measurements, particularly in moderator and coolant circuits as well as in environmental samples. The status of development of the field of "Tritium Measurement and Applications" was reviewed recently at a Symposium /17/ held in Bombay to mark the golden jubilee of the discovery of tritium in 1939.

8.1 Analysis of Acqueous Samples

The tritium levels in the electrolytes and other aqueous samples was measured by expert groups at the Isotope and Health Physics Divisions of BARC. Commercial liquid scintillation counting systems such as Packard Instruments Model 4530 or LKB Systems Model 1215 (RACKBETA–II) which provide automatic quench correction facilities were employed. ^{40}K free vials were used. Commercially available scintillation cocktail, INSTAGEL, was found most suitable as it gave minimum chemiluminiscence. Double distilled water was used for diluting samples to reduce PH level as well as quenching impurities. In some electrolyte samples chemiluminiscence effects entirely masked the true tritium signal. Fig.17 shows the chemiluminiscence decay curve of one such "difficult" sample which did not cool down even after several hours of dark adaptation and decay time.

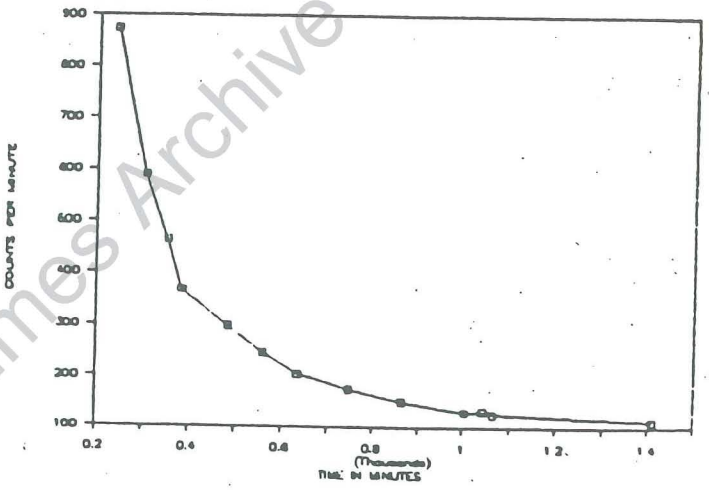

Fig.17 Chemiluminscence Decay Curve of a Severe Electrolyte Sample

Chemical and photon quenching effects were encountered in other samples resulting in compression of the tritium spectrum. In order to confirm that the spectral output of the electrolytic samples falls well within the tritium spectral region, an experimental sample which did not show any chemiluminiscence or quenching effects was spiked with a tritium standard and the pulse height spectrum was checked and compared with that of the test sample. In the case of LiOD based electrolytes a systematic study has been carried out to study the influence of alkalinity on the measurements.

The Pollution Monitoring Section of BARC has recently developed a flow detector for on line measurement of tritium levels in gas and liquid phases /17/. The flow cell has a volume of 15ml and is packed with 5 μm thick plastic scintillating fibres. A pair of photomultiplier tubes viewing from the sides and operating in ccincidence measure the scintillation rates, due to tritium.

8.2 β–particle and K X–ray Counting of Deuterated Ti and Pd Targets

The presence of tritium in the near surface region of deuterated Ti and Pd targets has been established through the technique of β particle and Ti K X–ray counting. The βs were counted using either a proportional gas counter or a plastic scintillator. In some of the strong sources pico amp levels of ionization current could be measured using an ionization chamber. The range of the tritium β^s (energy < 18.6 keV) in metallic samples is < 1mg/cm^2. Some of the β^s excite the characteristic K X–rays of Ti (~4.5 kev) whose mean free path for absorption in Ti is however 10mg/cm^2. Thus these X–rays are able to escape from deeper layers of the Ti than the β^s. Using a high resolution germanium detector or a 1mm thick NaI detector these soft X–rays can be conveniently counted.

8.3 Autoradiography

As mentioned already the technique of auto radiography has been employed very effectively at BARC to study the spatial distribution of near surface tritium produced in deuterated Ti and Pd targets. In order to obtain good resolution the samples were kept in contact with medical X–ray film and exposed overnight. The fogging of the film is due to the combined effect of tritium βs and the K X–rays excited in the host metal lattice. Sometimes a stack of two films was mounted close to the sample and it was observed that the second film also gives a similar but fainter image ruling out the suspicion that image formation could be due to mechanical (scratching of films) or chemical reduction effects.

9. CONCLUSIONS

Experiments carried out by a number of totally independent groups employing diverse experimental set ups have unambiguously confirmed the production of neutrons and tritium both in electrolytically loaded and gas loaded Pd/Ti lattices.

Tables I & II present in a nut shell the main results of the BARC electrolysis experiments. It may be noted that in all 22 cells/experiments have yielded excess tritium varying over a wide margin of 10^{10} to 10^{16} atoms. Roughly half of these may be described as having been "doubly successful" since in these both neutrons and tritium were measured. The main conclusions to emerge from the electrolysis experiments may be summarized as under:

(a) The most important observation is the surprisingly low neutron–to–tritium yield ratio, first reported by us at the Karlsruhe meeting in July 1989 /3/. 8 out of the 11 doubly successful cells of Table I have given values in the region of 10^{-6} to 10^{-9} for this ratio, while two experiments have given a comparatively larger value of 10^{-3} to 10^{-4}. These ratios may be considered as overestimates since in most of the experiments the tritium escaping with the electrolytic gases has not been accounted for.

(b) The Trombay electrolytic experiments have also convincingly demonstrated that both neutrons and tritium are generated concomitantly. This is evident from the sharp increase in the tritium concentration of the electrolyte immediately after a large neutron burst in several of the experiments.

(c) Another significant observation pertains to the specific charge passed per unit of cathode surface area, namely amp–hrs/cm^2 at the time of the first neutron burst. This quantity which may be called the "switching on charge" is seen to be in the range of 0.6 to 3.2 amp–hrs/cm^2 in 8 out of the 11 experiments of Table I. In the remaining three cells (all of which have used LiOD as electrolyte) the switching on charge is an order of magnitude or more higher.

(d) It is significant that in all the five experiments which used 5 M NaOD as electrolyte the switching on charge was $\leq$ 3 amp–hrs/cm^2. In the isolated instance where Li_2SO_4 was used the switching on charge was the lowest namely 0.15 amp–hr/cm^2 among all the experiments reported in Table I.

(e) In 8 out of the 11 cases of Table I the first neutron burst has occurred on the very first day of electrolysis, in fact within 9 hrs of commencement of electrolysis (except for experiment #7 where it occurred after ~24 hrs). This seems to be a unique feature of the Trombay results.

(f) It is also worth noting that one amp–hr or 3600 coulombs corresponds roughly to the charge carried by the deuterons required to load a few grams of Pd (associated with each cm^2 of cathode surface) to a (D/Pd) ratio of ~0.6. In practice since only a fraction (10 ~ 30%) of the deuterons bombarding the cathode actually get absorbed in it, the experimentally observed switching on charge of $\leq$ 3 amp–hr/cm^2) is consistent with the common sense expectation that a (D/Pd) ratio of atleast 0.6 should be achieved before nuclear processes involving

deuterons can be expected to take place.

(g) Except for the Ti–SS cell (Cell#1 of Table I) in all the other ten cells neutrons are produced in one or more large bursts of magnitude several times the background values. But in all cases after a limited period of nuclear activity the cells becomes inactive, no matter for how long the electrolysis is continued.

(h) Inspite of the wide disparity in cell designs, it is observed that the specific neutron yield i.e the integrated neutron yield per unit area of cathode surface, lies in the range of 10^4 to 10^5 n/cm^2 except for the two experiments where a Nafion membrane was present between the cathode and anode (Cells # 9 & 10). The specific neutron yield in these two cases is 1 or 2 orders of magnitude higher.

(i) The specific tritium yield (see Tables I & II) has shown a greater overall variation ranging from 4×10^9 to 1.7×10^{14} t/cm^2. In 10 of of 22 cells it is in the range of 10^{12} to 10^{14} t/cm^2 while most of the other cells have given values in the region of 10^{10} to 10^{12} t/cm^2.

(j) The BARC teams generally operated only one cell at a time. The overall "success rate" defined as the percentage of cells which produced tritium or neutrons in relation to the total number of cells operated is estimated to be more than 70%. The groups who used NaOD as electrolyte had perhaps an even higher success rate.

(k) The BARC experiments possibly include the largest sized electrolytic cold fusion cells (measured in terms of either cathode surface area (300 cm^2) or total current (100 amps)) to have been employed so far.

(l) Unfortunately it has not been possible to conclusively establish whether the neutron and tritium producing reactions occur only on the surface of the electrode or over the whole volume of the cathode. But the delayed appearance of additonal tritium in the electrolyte, at times even when the cell was off, indicates that tritium slowly leaches out from the inner regions of the electrode, giving some credence to the volume effect theory.

The gas phase experiments of BARC have spanned a variety of novel approaches and diagnostic techniques. The observation that RF heating of titanium targets in D_2 atmosphere promotes tritium production is interesting. The large amount of tritium (10^{16} atoms) found on the top end surface of the central titanium electrode of a plasma focus device, particularly the very impressive high resolution autoradiogrpah, is puzzling indeed. Finally the unexpectedly large levels of tritium in decades old deuterated titanium targets adds to the pool of puzzling results.

On the whole however, the results obtained by a number of independent experimental groups at BARC during the first year of the 'cold fusion era' has provided ample evidence of the occurrence of anamolous nuclear processes in Pd and Ti lattices loaded with deuterium.

ACKNOWLEDGEMENTS

We are very grateful to all the authors of the individual BARC cold fusion papers for readily supplying experimental details and results for the preparation of this overview paper. We wish to thank Drs R.M. Iyer and T.S. Murthy for many fruitful discussions. The considerable help rendered by A. Shyam, T.C. Kaushik, R.K. Rout, V. Chitra, S. Ranganekar and D.V. Periera in the time consuming task of editing and formatting of this manuscript is warmly acknowledged.

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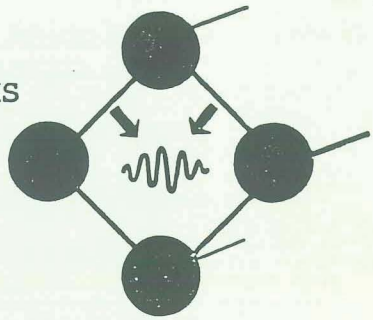
ANOMALOUS NUCLEAR EFFECTS IN DEUTERIUM/SOLID SYSTEMS
an international progress review
October 22-24, 1990



July 19, 1990 (second revised mailing)

Dear Colleague:

An international workshop is planned for October 1990 to review progress on "Anomalous Nuclear Effects in Deuterium/Solid Systems". The workshop will provide an opportunity to exchange the latest information on the specifically nuclear effects associated with studies of "cold fusion". A particular effort is being made to involve researchers from Japan, the Soviet Union, India, China and Europe. The workshop will be held at the Brigham Young University campus in Provo, Utah, U.S.A. The emphasis will be on nuclear physics and geophysical experimental data, with theoretical contributions also solicited. The subjects of calorimetry and electrochemistry will be discussed only as adjuncts to observations of nuclear reaction products.

The sponsors for this workshop are the Electric Power Research Institute, Brigham Young University, and the U.S. Department of Energy. The organizing committee includes Dr. V.B. Belyaev (Dubna Laboratory, USSR), Prof. S.E. Jones (BYU), Prof. K. Nagamine (University of Tokyo), Dr. F. Scaramuzzi (Frascati, Italy), Dr. M. Srinivasin (Bhabha Institute of India), and Dr. D.H. Worledge (EPRI).

We would very much welcome your participation. The sessions planned are as follows:

1. Anomalous Neutron Production in Deuterium/Solid Systems

   We request papers that support or refute the anomalous production of neutrons in such systems. Only those experimental results that include comparative blank runs and characterize the natural background should be presented.

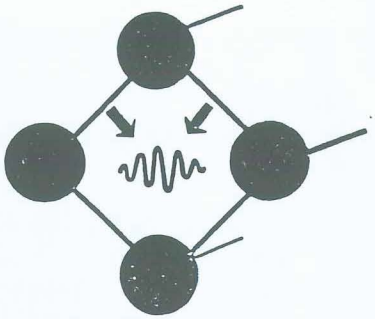
2. Measurement of Electromagnetic Radiation or Energetic-Charged Particles in Deuterium/Solid Systems

   Papers that involve X-ray, gamma or charged particle detection are requested.

3. Anomalous Tritium Production in Deuterium/Solid Systems

   Papers are requested that support or refute the anomalous production of tritium in such systems. Only those experimental results that include comparative blank runs and documentation of initial tritium content should be presented.

ANOMALOUS NUCLEAR EFFECTS IN DEUTERIUM/SOLID SYSTEMS
An International Progress Review
22-24 October 1990



July 18, 1990

Dear Colleague,

The organizers of the progress review on "Anomalous Nuclear Effects in Deuterium/Solid Systems" wish to thank those who have already indicated their interest to participate. Nearly one hundred people have responded to the meeting announcement, including scientists from the Soviet Union, Japan, Europe, India, the China, Korea, Argentina, Canada and the United States. We feel that this is a proper-sized group for effective information exchange, although we do not wish to discourage additional participation. Participants include those reporting anomalous nuclear effects in condensed matter as well as those who have found no such effects. We welcome all interested parties, as this promotes careful, peer-reviewed scientific progress. The workshop format, along with ample opportunities for informal discussions, will encourage free exchange of ideas and networking of researchers.

We have asked that abstracts for talks be submitted before September 15, 1990, to allow us sufficient time to organize the sessions while allowing the abstracts to be as up-to-date as possible. Please submit abstracts to Prof. Steven Jones, Dept. of Physics, Brigham Young University, 176 ESC, Provo, Utah, 84602, U.S.A., fax: (801) 378-2265.

The information provided below will help with transportation, dining and lodging arrangements. For assistance in these areas, you may contact BYU Conferences, Nuclear Fusion, 154 Harman Building, Provo, Utah, 84602, U.S.A., (801) 378-4851. For assistant on other subjects, contact Nanette Hamm at (801) 378-4516.

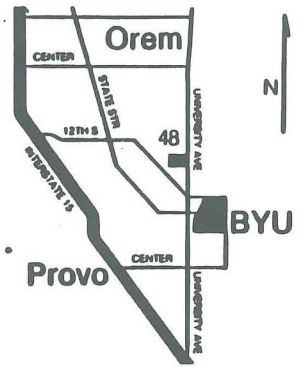
Registration
The administrative fee for the progress review will be $90 U.S. Please register by mail using the enclosed form.

Registration materials can be picked up Monday morning, October 22, 1990, beginning at 8:00 a.m. at the BYU Conference Center (CONF), Room 2297. (No activities are planned for Sunday evening.)

Transportation
The Salt Lake City (SLC) International Airport is approximately 45 miles (75 km) north of Provo. One may drive south from the airport on Interstate 15 to Provo (maps enclosed). Alternatively, shuttles from the SLC Airport to and from Provo hotels are provided by Key Transportation ((800)-678-2360, or (801) 328-2360), with a desk at the SLC airport. Cost is $15 one way, $27 round-trip. Reservations should be made in advance.

Transportation between the BYU Conference Center and the four motels recommended for the conference (see below) will be provided. Transportation to Robert Redford's Sundance Resort in Provo Canyon for the banquet will also be provided.

BRIGHAM YOUNG UNIVERSITY CAMPUS



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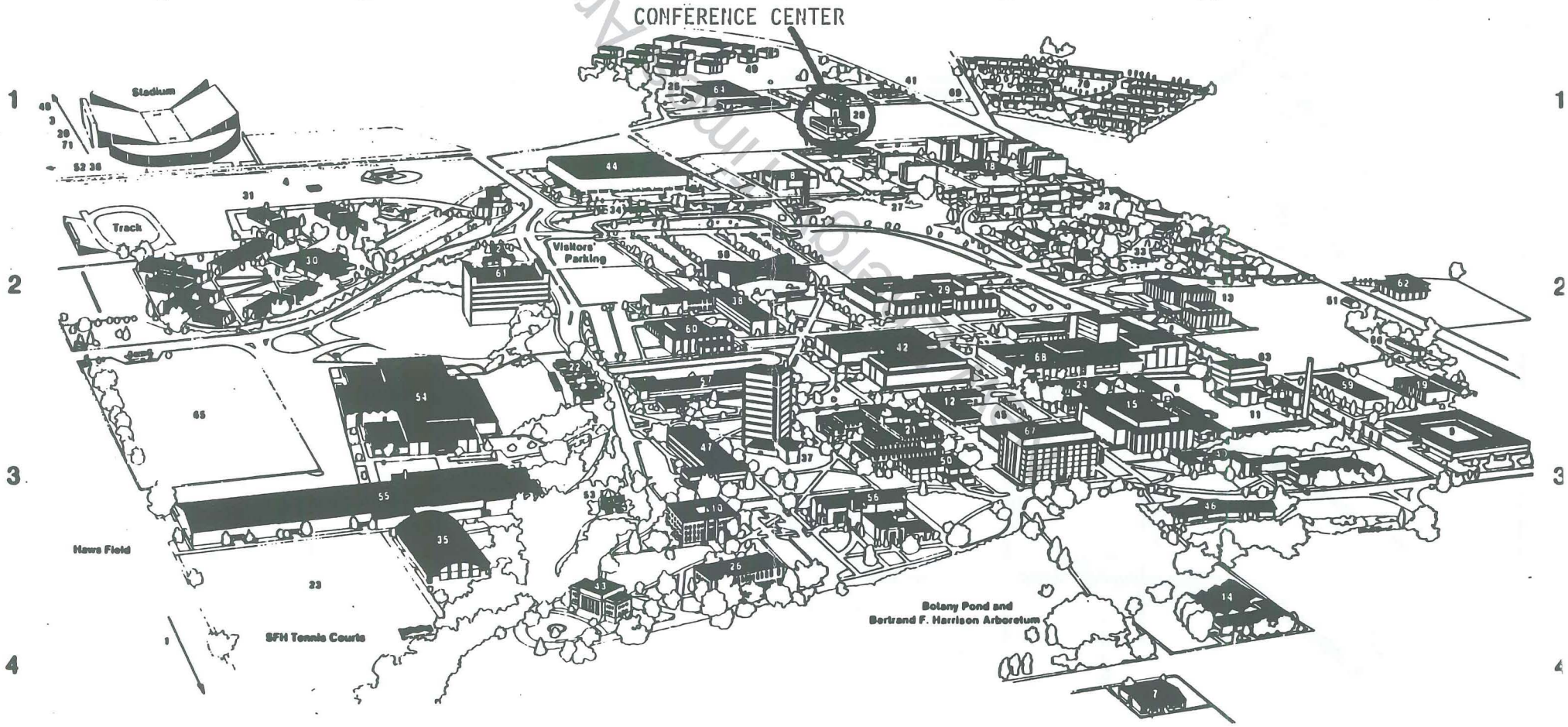
70. Wymount Terrace (WT) ....
71. Wyview Park ....

See current BYU Telephone Directory for additional buildings, abbreviations, and addresses.



21 JUN 89 6:71 R.L. GARWIN

DEPARTMENT OF ENERGY
WASHINGTON, DC 20585

LABORATORY MANAGEMENT DIVISION (CLAFLIN)
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ENERGY RESEARCH ADVISORY BOARD (STONE)

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DATE: June 20, 1989

TO: COLD FUSION PANEL MEMBERS
(Name/Office Symbol/Telephone No.)

FROM: BILL WOODARD, DEPT. OF ENERGY, (202) 586-5767
(Name/Office Symbol/Telephone No.)

This transmittal consists of 5 pages, (excluding cover sheet).

John Huizenga
Norman Ramsey
Allen Bard
Jacob Bigeleisen
Howard Birnbaum
Michel Boudart
Clayton Callis
Mildred Dresselhaus
Larry Faulkner
T. Kenneth Fowler
Richard Garwin
Joseph Gavin, Jr.
William Happer, Jr.
Darleane Hoffman
Steven Koonin
Peter Lipman
Barry Miller
David Nelson
John Schiffer
John Schoettler
Dale Stein
Mark Wrighton
David Goodwin

062089.WLW

Lawrence Livermore National Laboratory

June 16, 1989

Dr. William Woodard
Energy Research Advisory Board
U.S. Department of Energy
Washington D.C.

Dear Dr. Woodard,

Please find, as you requested, a short summary of the experimental and theoretical program at the Lawrence Livermore National Laboratory on the subject of cold fusion. Our first reactions to the initial reports of Pons and Fleischmann were those of surprise because they were inconsistent with our present understanding. However the importance of such a new approach to fusion was so great that a very large number of our scientists began in earnest to examine the reported experiments. It rapidly became clear that there were two classes of experiments.

(1) Those that measured excess heat from D_2 loaded Pd electrodes in an electro chemical cell. (Pons and Fleischmann, Srinivasan, Huggins,....)

(2) Those that measured a very small number of excess neutrons from D_2 loaded Pd electrodes in electrochemical cells, however these cells produced no excess heat. (Jones et al, Frascatti laboratory results)

Our calculations and theoretical research have shown that there is no reason to believe that D_2 in a palladium matrix should fuse and cause the observed heat or the observed neutrons. Furthermore we have been unable to measure enhanced heat production in D_2 loaded palladium-electrode electrochemical cells. In one case (Srinivasan at Texas A&M) we have examined palladium from cells that appeared to have generated excess heat but we see no evidence of fusion -- no He^3 or He^4, tritium, or neutrons.

In our experiments that fall in class (2) above, which include the initial experiments of Jones et al, we have measured no neutrons. In experiments with D_2 pressured titanium metal, we, LANL, Frascatti and others may have seen bursts of a few neutrons, but we are not yet convinced of the accuracy of these measurements. While we are convinced that this class of experiments has produced no useful fusion or accompanying heat, there are some curious observations that should be explained, but at a low funding level.

In summary we believe that at the National Laboratory level there is little additional work to do in the area of cold fusion. Please see the attached sheet for a more detailed explanation of our results. Thank you for this opportunity to describe our results.

Sincerely,

John H. Nuckolls
Director